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Vol. 29 **December 1957** **No. 6**

DISCUSSION, PROCEEDINGS V. 53

Discussion of papers published in the January through June, 1957, JOURNALS appears in this issue, along with title page, errata, contents, and indexes for *Proceedings* V. 53. Discussion of papers in the first half of the JOURNAL year (July-December 1956) appeared in the June, 1957, JOURNAL

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ERRATA and ADDENDA

The following additions and corrections should be made in the indicated papers and reports of *Proceedings* Volume 53, July 1956 through June 1957.

Title No. 53-1 "Structural Design Considerations for Pavement Joints"

p. 8—Crack shown in formed groove type of joint should extend above deformed tie bars to sealing compound.

p. 9—Line three of third paragraph should read in part "they should have," in place of "they must have."

p. 21—In Fig. 10, legend for uppermost curve should read RATIO f_b'/f_c'. For the curve labeled ULTIMATE COMPRESSIVE STRENGTH OF CONCRETE IN MARCUS TESTS the notation following should be (f_c').

* * * *

Title No. 53-2 "Shearing Strength of Reinforced Concrete Slabs"

p. 56—Eq. (i) should read:

$$P_{flex} = 4m\frac{a-r}{2}\sqrt{2}\frac{\sqrt{2}}{a-r} + 4mr\frac{\frac{1}{2}}{\frac{a-r}{2}} = 4m\left\{1 + \frac{1}{\frac{a}{r} - 1}\right\}$$

The two sentences under Eq. (i) should read:

"The equation giving the smallest P_{flex} will govern. Thus Eq. (h) applies when $r \geq a/2$, and Eq. (i) applies when $r \leq a/2$."

EDITOR'S NOTE—Eq. (h) and (i) were not used at any time in the analysis of the test results. Therefore, the error in Eq. (i) has no bearing on the conclusions of the investigation.

* * * *

Title No. 53-4 "Earthquake Resistant Design Based on Dynamic Properties of Earthquakes"

p. 96—The heading of Column (3) Table 2 should be $W\bar{x}$.

* * * *

Title No. 53-7 "Considerations for Construction of Subgrades and Subbases for Rigid Pavement"

p. 146—Third line from the bottom, delete "appears to be" and replace with "is not."

p. 148—Next to the last sentence in the second paragraph, delete "without a detrimental dust condition."

* * * *

Title No. 53-11 "An Expression for Creep and Its Application to Prestressed Concrete"

p. 212—Second exponent in Eq. (12) should be C_1 not $C1$.

p. 211—Third line, k/t should read K/t.

* * * *

Title No. 53-14 "Lining of the Tecolote Tunnel"

p. 258—Second line of third paragraph, change "shaft" to "tower."

p. 262—Fourth line, fourth paragraph, the dimension should be 4 ft 4 in.

* * * *

Title No. 53-15 "Theory of the Stresses Induced in Reinforced Concrete by Applied Two-Dimensional Stress"

p. 280—In Fig. 2, angles labeled γ and $\pi/2 - \gamma$ should be α and $\pi/2 - \alpha$, respectively.

* * * *

Title No. 53-16 "Some Effects of Carbon Dioxide on Mortars and Concrete"

p. 299—Substitute the graphs below for Fig. 2a and 2b.

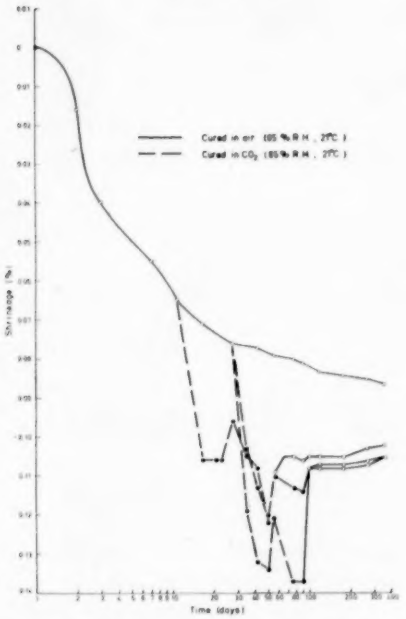


Fig. 2a—Shrinkage of mortars which have been kept in carbon dioxide after various air curing

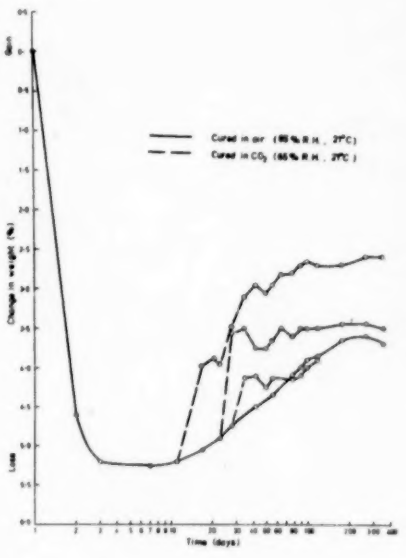


Fig. 2b—Change in weight of mortars for which shrinkage is shown in Fig. 2a

* * * *

Title No. 53-24 "Considerations in the Selection of Slab Dimensions"

p. 446—The expression $\dfrac{E_c\, h^3}{\dfrac{k\,(a_e + b_e)^4}{2}}$ in Eq. (4) should read $\dfrac{E_c\, h^2}{k\left(\dfrac{a_e + b_e}{2}\right)^4}$

* * * *

Title No. 53-25 "Guide for Ultimate Strength Design of Reinforced Concrete"

p. 465—Eq. (22), the denominator of the first fractional term following equal sign should be

$$\frac{0.15\, t}{d} + 1 \text{ instead of } \frac{0.15\, D}{d} + 1$$

p. 468—Substitute the following for Fig. 6:

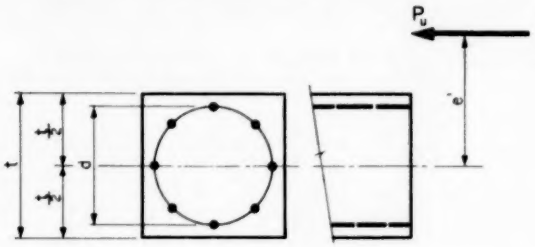


Fig. 6

p. 468—Substitute the following for Fig. 7:

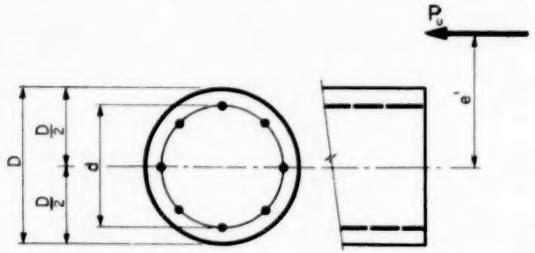


Fig. 7

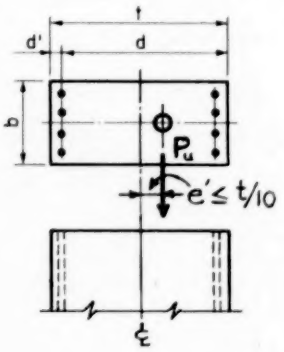
p. 469—Eighth line from the bottom, " . . . planes of the existing moment" should read "planes of the resisting moment."

p. 470—Delete last prime sign at end of Eq. (33).

p. 471—Lines 6 and 7 should read "from the formulas for concentrically loaded and eccentrically loaded short columns and combined bending and axial compressive load or from Charts 2-15."

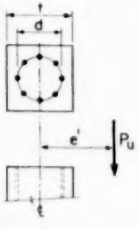
p. 474—Add "psi" at the end of Line 33.

p. 477—Substitute the following for the corresponding portion of the upper left corner of Chart 2.



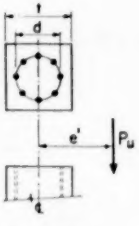
p. 478—Chart 3 applies to eccentricities less than or equal to $t/20$, not $t/10$.

p. 483ff.—Substitute the following for the corresponding parts in the upper right corners of Charts 8-15, inclusive.



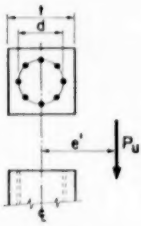
d/t=0.90

Chart 8



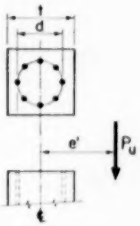
d/t=0.80

Chart 9



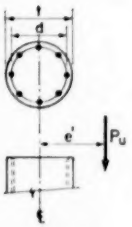
d/t=0.70

Chart 10



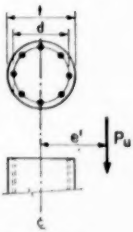
d/t=0.60

Chart 11



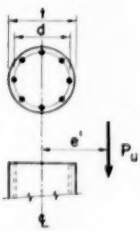
d/D=0.90

Chart 12



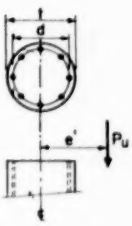
d/D=0.80

Chart 13



d/D=0.70

Chart 14



d/D=0.60

Chart 15

* * * *

Title No. 53-27 "Insulating Concretes"

p. 509—Add to footnote identifying author, "formerly materials engineer, National Bureau of Standards, Washington, D. C."

p. 530—Add the following paragraph on acknowledgments:

ACKNOWLEDGMENTS

The preparation of this paper was done at and supported by the National Bureau of Standards, Structural Engineering Section, Washington, D. C.

The author is indebted to many persons for supplying information used in preparing the paper. In particular, gratitude is due H. E. Robinson and R. Waterhouse of NBS who gave valuable advice concerning the sections of the paper on thermal conductivity and acoustics, respectively. Inasmuch as the author left the National Bureau of Standards shortly after preparing the manuscript, former colleague David Watstein, chief of the Structural Engineering Section of the Building Technology Division, aided in the editorial processing of the paper.

* * * *

Title No. 53-29 "Particle Interference in Concrete Mixes"

p. 533—" . . . grading analyzed in Table 2" should read "grading analyzed in Table 1."

* * * *

Title No. 53-31 "Mackinac Bridge Pier Construction"

p. 595—Last paragraph under ORGANIZATION, insert: "Lee Turzillo, former executive vice-president of the Prepakt Concrete Co., was active in planning and supervision of concrete placement work until December 1955, when he formed his own firm, Lee Turzillo Contracting Co., Cleveland.

* * * *

Title No. 53-35 "Laboratory Investigation of Rigid Frame Failure"

p. 643—Last line, "1381" should be "1281."

p. 647—Eq. (3) should read $M = aP_a + jdC$

* * * *

Title No. 53-39 "Proposed Recommended Practice for Design of Concrete Pavements"

p. 726—The expression $\dfrac{\dfrac{E_c\, h^3}{k\,(a_c + b_c)^4}}{2}$ in Eq. (4) should read $\dfrac{E_c\, h^3}{k\left(\dfrac{a_c + b_c}{2}\right)^4}$

p. 727—Change "Loop A" and "Loop A'" on Fig. 403(c) to "Load A" and "Load A'."

p. 728—Correct formula for relative stiffness below Table 403(d) so that the fourth root sign applies to both numerator and denominator.

p. 741—Eq. (14) should read $A_s = \dfrac{F\, L\, w}{2f_s}$

p. 742—The symbol K denoting subgrade reaction should be changed to lower case k.

p. 743—The symbol K denoting subgrade reaction should be changed to lower case k.

* * * *

Title No. 53-47 "Shear Strength of Reinforced Concrete Frame Members Without Web Reinforcement"

p. 857—The second equation on the page should read

$$10^4 K_{e_u} = \frac{1.116\,\dfrac{a}{d} + 0.174}{\dfrac{a}{d} - 0.872} \leqq 3\,\frac{a}{d} \text{ (always larger than zero)}$$

* * * *

Title No. 53-52 "Proposed Revision of Specifications for Concrete Pavements and Concrete Bases"

p. 943—Section 1401(d), next to the last sentence should read: "Such measurement shall be made at intervals of not less than 200 ft."

* * * *

Title No. 53-53 "Tests of a New Method for Evaluating Volume Changes of Concrete Masonry Units"

p. 954—In Fig. 4, the 67.3 percent M.C. refers to Curve 2 *after* 1 day of drying. The indicator line was incorrectly drawn.

p. 956—In Fig. 6, the ordinate should read LENGTH CHANGE instead of VOLUME CHANGE.

* * * *

Title No. 53-54 "Elastic Design of Prestressed Sections in Flexure by Charts or Tables"

p. 971—Fifth line from the bottom, concluding mathematical expression should read:

$$-(I/h)f_{1d_1} - M_1$$

p. 972—Third equation from the bottom of page, first term should read: $\frac{i f_{2d}}{a}$

p. 973—Eq. (14_1) should read: $N = \frac{\nu L^2}{f_1 h} = ey_2 - \mu \ldots\ldots\ldots\ldots (14_1)$

p. 986—Denominator of the second term of Eq. (c) should be $Z_1 h^3$.

* * * *

Title No. 53-58 "Tables for Circularly Curved Horizontal Beams with Symmetrical Uniform Loads"

p. 1037—Eq. (23) should read: ϕ, in radians $= \sqrt{6\ N/(N+1)}$

* * * *

Title No. 53-59 "Mortar- and Concrete-Making Properties of Natural Sands Related to Their Physical Attributes"

p. 1041—Author's name should be spelled ZIETSMAN.

p. 1052—Square brackets on both right and left hand side of Eq. (10) indicate absolute quantities, not algebraic differences; i.e., they are used in a positive sense in solving the equation, regardless of whether the quantity within the brackets is positive or negative.

The equation may better be written as follows:

$$\left| \frac{W_4}{10} - 37.2 \right| + 9 = \sqrt{9^2 + 13.2^2 - \left(13.2 - \left| 100J - 62 \right| \right)^2} \ldots\ldots (10)$$

* * * *

Title No. 53-61 "Diagonal Tension Strength of Reinforced Concrete T-Beams with Varying Shear Span"

p. 1070—In Column 4 of Table 3, delete the prime sign following A_s.

* * * *

Title No. 53-64 "Mass Concrete Control in Detroit Dam"

p. 1150—Paragraph 4, temperature differential is 25 F, not 250 F.

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Discussion of a report by ACI Committee 315:

Proposed Manual of Standard Practice for Detailing Reinforced Concrete Structures*

By RICHARD W. SOUZA and COMMITTEE

By RICHARD W. SOUZA†

Chapter 5 of the new edition of the manual by ACI Committee 315 has made many valuable suggestions to improve coordination between the design engineer and the reinforcing steel detailer. However, the committee has not mentioned the most troublesome portion of the structure, the foundations. Variations in soil conditions and in the excavation work itself often cause the foundation to be located at elevations different from those specified in the engineering drawings. The detailing of foundation walls often requires more time than the detailing of the superstructure; and, it is always the foundation steel which is needed on the job immediately.

While these points are recognized by all experienced engineers, many drawings show foundation steel details which are complicated and inflexible. By simplification and flexibility the engineer can expedite the job and be assured that all sections will be properly reinforced in spite of minor variations which occur on all foundation work. Suggested methods for achieving flexibility and simplicity are:

1. Extension of footing and slab dowels farther than the necessary lap requirement to allow for variations in footing grades.
2. Use of spliced vertical bars with sufficient lap for variations in grade, where dowels are not called for.
3. Sloping of horizontal bars at changes in continuous footing elevations and under brick shelves.
4. Inclusion of slab and beam dowel details on the foundation sections to help prevent omission of these important items.
5. Use of plain concrete for small size individual footings.

Although the application of these suggestions will result in the use of more steel, cost of this additional steel should be offset by the reduction of detailers' and checkers' time and the elimination of difficulties in the construction itself.

COMMITTEE CLOSURE

Mr. Souza's comments relate to the problem which arises on almost every construction; namely, that the footing courses when actually installed to suit

*ACI JOURNAL, Jan. 1957, *Proc.* V. 53, p. 617. Disc. 53-33 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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the soil conditions encountered will often be at elevations somewhat different from those established by the bidding drawings. Most of the experienced detailers attempt to provide flexibility by various methods of splicing and by the location of bars in the structure, but, to a considerable extent, a detailer is guided by local practices. For example, Mr. Souza's Item 1 would work well if wall verticals are wired to the extended dowels but would waste material if the wall verticals rest directly on the footing course anyhow.

Suggestions for detailing for maximum simplicity and repetition and for providing for variation and flexibility appear throughout the manual.

For reader convenience and to retain continuity of discussion, all discussion of the three-paper series on the AMC warehouse failures is published together.

Disc. 53-34
53-35
53-36

Discussion of papers by Boyd G. Anderson, Richard C. Elstner and Eivind Hognestad, and Reinhart R. Lunoe and George A. Willis:

Rigid Frame Failures*

Laboratory Investigation of Rigid Frame Failure*

Application of Steel Strap Reinforcement to Girders of Rigid Frames, Special AMC Warehouses*

By P. W. ABELES, LOUIS BALOG, K. HAJNAL-KÓNYI, ERNEST H. HARDER, JOSEPH E. PERRY, J. R. SHANK, and AUTHORS

By P. W. ABELES†

The shear failure described in "Rigid Frame Failures," by Anderson, has proved that the provision of stirrups, disregarding the magnitude of shear stress, is essential with ordinary reinforced concrete constructions. With regard to shear, there are in principle three different conceptions in the various codes for reinforced concrete. In Germany, the entire shear in the concrete over the entire length of the member must be taken up by steel when the shear stress in the concrete exceeds the permissible value. In the United States the cooperation of the concrete is not excluded and only the shear stresses exceeding the permissible value have to be carried by steel. Between these two extremes, the codes of Great Britain and other European countries require that the entire shear stress in the concrete over the length at which it exceeds the permissible value should be taken up by steel, but it is not specifically demanded that steel must be provided in the remaining portion. Nevertheless, it would be considered as bad practice anywhere in Europe to design ordinary reinforced concrete constructions without the provision of stirrups over the entire length, disregarding the code requirements. It is therefore surprising that these ideas have not been introduced into the United States and Canada, as an appreciable number of European engineers of certain experience are continually settling there.

In continuous construction the points of contraflexure are not confined to specific positions, but depend on the extent of loading and will therefore move between certain limits. Consequently, tensile stresses may occur there in a section at the upper and lower sides during different loadings and, therefore, the provision of stirrups and of longitudinal top and bottom bars is par-

*ACI JOURNAL, Jan. 1957, *Proc.* V. 53, pp. 625, 637, 669, respectively. Disc. 53-34, 53-35, and 53-36 are a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.
†Structural and Civil Engineer, London, England.

ticularly important in the neighborhood of the points of contraflexure. Incidentally, if stirrups had been provided it would have been essential to place bars at the outer corners of the section over the entire length of the horizontal members of the frame and it is most likely that the termination of the upper bars just at the points of contraflexure would have been avoided.

Shear failure in reinforced and prestressed concrete occurs suddenly, without previous warning, and it is essential to take certain precautions. Mr. Anderson mentioned that "the nominal shear stress . . . was well below the shear capacity" and "that the diagonal tension type failures occur after bond slip in the bars." It is thus the principal tensile stress in the concrete that matters. This problem is of particular importance when shear and bending cracks occur simultaneously and is especially aggravated in such a case in which sufficient tensile reinforcement is missing as at points of contraflexure. This question of shear and combined bending applies not only to reinforced concrete but likewise to prestressed concrete, as the writer has already shown.* These tests proved that shear failure may take place with concrete of high strength at relatively low nominal shearing stresses if high flexural stresses causing heavy cracking occur simultaneously. However, it is possible to obtain quite safe prestressed concrete constructions without stirrups if, under ultimate load conditions, the maximum resultant principal tensile stress is below the concrete tensile strength and thus the development of cracks is avoided. In the light of this, it is surprising that in the United States the provision of stirrups over the entire length is apparently required only with prestressed concrete, notwithstanding the fact that the conditions are greatly improved by the precompression due to effective prestress, while the provision of stirrups or bent-up bars was previously reduced to a minimum with ordinary reinforced concrete and it is still less than the practice in Europe, even after the revision mentioned by Mr. Anderson.

The collapse of the frame structure described is certainly regrettable but it has given rise to interesting investigations, discussed by Elstner and Hognestad in "Laboratory Investigation of Rigid Frame Failure," which showed that in addition to principal stresses due to shear, tensile shrinkage stresses must also have played an important part in the failure. This may also give rise to further research and general improvement in the construction. Thus perhaps further results may ensue from these investigations which would never have been obtained without this research imposed by the failure.

By LOUIS BALOG†

Design for shear

The fundamental behavior of concrete girders has been recognized from the earliest application of reinforced concrete for long-span girder construction. Unlike in the design of girders from homogeneous materials, shear requires

*Abeles, P. W., "The Use of High Strength Steel in Ordinary Reinforced and Prestressed Concrete Beams, *Final Report*, Fourth Congress, International Association for Bridge and Structural Engineering, Cambridge, 1953.

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primary consideration in the design of the reinforced concrete girder. If full shear reinforcement is not provided where diagonal tension cracks develop, the carrying capacity of the girder will be governed by shear. The multiple role of the full length shear and longitudinal reinforcement in taking care of the loads and of the effects of shrinkage and temperature were also quantitatively recognized.[1,2]

Never, before the design of these warehouse frames, has it occurred that 66-ft span girders, having no shear reinforcement in 54 percent of their length and no longitudinal steel, like the Wilkins type girders, or having a web to main steel ratio of 0.16 in 28 percent of their span adjacent to the supports, like the Robins type girders, were considered to be satisfactory. The Wilkins girders failed near the 0.3 span length point where the dead load shear was 75 psi; the Robins girders failed near the 0.2 span length point where the dead load shear was 100 psi. European specifications required full shear reinforcement at both these sections.

English specifications[3] of July 16, 1915, allowed $v_c = 0.1\ f_c = 60, 65, 70, 75$ psi and $v = 3\ v_c$ for four mixes of concrete. They required that wherever the value of the unit shearing stress exceeded the allowed value of v_c that web reinforcement must be provided for the total shear. Manning stated: "To increase the bending strength at the expense of the shear strength is to reduce the load-carrying capacity of the beam."[2] The Swiss specifications[3] of Nov. 26, 1915, required that web reinforcement must be provided for the total shear wherever v_c is larger than 43, 50, and 57 psi in railway bridges, highway bridges, and building structures, respectively. Other continental specifications were similar to the Swiss until 1925 when full shear reinforcement was introduced in Germany. Greece and Bulgaria accepted the German code literally. The Russian code of Feb. 24, 1926 required that bent bars and stirrups must be provided for all diagonal tension in girders, as in Germany.[3]

Numerous published computations and designs prove that in Italian and Russian long-span girders the entire shear was covered by steel even more meticulously than in Germany. Data on long-span girders built all over the world reveal similarity in design practice as to the total amount of shear reinforcement provided, except for the long-span girders built in the United States, especially during the past decade.

The role of shear reinforcement and longitudinal steel in the regions of contraflexure, in assuring the flexural carrying capacity of girders, was established by the tests of O. Graf and E. Mörsch in 1927. It was demonstrated by L. Herzka in 1929 that the omission of longitudinal reinforcement in these regions results in the development of shrinkage cracks, even if diagonal tension would not cause cracks.[4]

Laboratory tests

These investigations reported by Elstner and Hognestad reproduced the failures of Wilkins and Robins girders on laboratory specimens and estab-

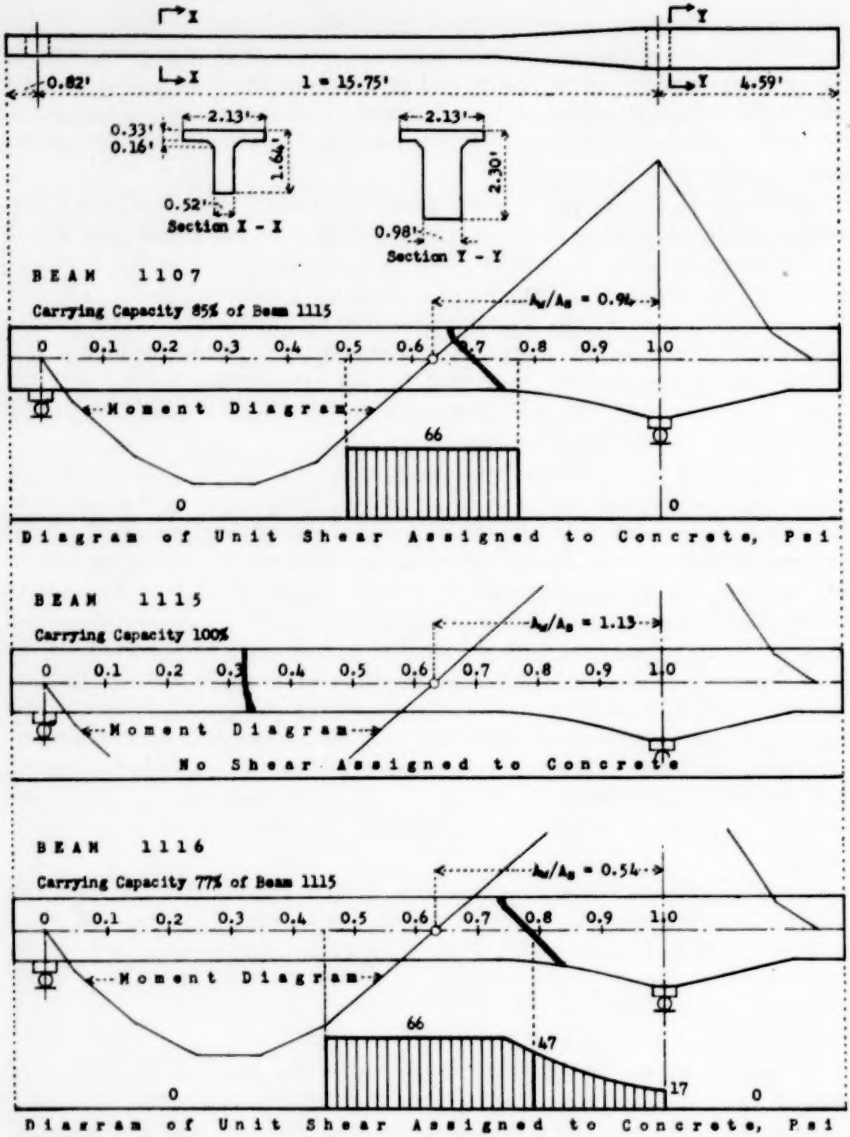

Fig. A—Dimensions, stresses, and types of failure for beams tested by Mörsch

lished the carrying capacity of specimens representing the 1955 design and design by ACI 318-56. From the design point of view the most important result of this work is conclusion No. 5, p. 668, stating that "Adequate safety in structures of the Wilkins type may be assured . . . by sufficient web reinforcement . . . sufficient extension of both positive and negative longitudinal

reinforcement well beyond the extreme positions of the point of contraflexure . . . " This conclusion is literally the same as that of E. Mörsch, derived from testing similar specimens of T-section in 1927.[5] Data on these test beams (No. 1107, 1115, and 1116) are shown in Fig. A.

Although the writer does not wish to assign any undue precision in predicting the carrying capacity of specimens by his empirical equation, $C_{red.}$ (percent) = 100 $[2\ (A_w/A_s) - (A_w/A_s)^2]$, the ultimate loads obtained by the tests and by this equation are presented in Table A for comparison.

Comparison of the actual A_w/A_s ratios of the Wilkins and Robins girders with those of the specimens listed in Table A indicate that the test beams contained, relative to their main reinforcement areas, about 10 percent more shear reinforcement than the prototypes. By comparing the ultimate load of the Wilkins and Robins specimens, it is seen that the 1954 revision did not increase the ultimate load, verifying the writer's statements to this effect.[6]

Unlike in laboratory specimens, large axial tensions are innate in the prototypes. For this reason the 1955 design and the ACI 318-56 specimens should have been subjected also to axial tension and flexural loading. The deficiency of ACI 318-56 seems to be confirmed by the behavior of Beam 6. Despite the omission of axial tension stress, Fig. 9, p. 655, indicates diagonal failure close to the section where stirrups were discontinued. The necessity of full length stirrup reinforcement is confirmed by the 1955 design, which contains significantly more shear and longitudinal steel than Beam No. 6.

The 1955 design has bar sizes different from the 1954 design (Robins type) and it is not described in any of the references given on p. 668. As published in the Jan. 12, 1956, issue of *Engineering News-Record*, p. 22, it contains 0.4 percent and 0.2 percent shear steel in 69 percent and 31 percent span length, respectively. This shear steel is 13 percent and 24 percent more in the haunch length and total length, respectively, than required by ACI 318-56. Beam 4, designated as 1955 design, contains much more shear steel than the prototype; in fact, it contains more than full shear coverage as indicated by the data in Table A.

The shear reinforcement of Beam No. 4 was not described completely on p. 653: "0.67 percent close to the supports, 0.60 percent in the contraflexure region, 0.18 percent at midspan;" therefore, it was assumed that 0.67, 0.40,

TABLE A—LABORATORY TESTS OF WILKINS, ROBINS, 1955 DESIGN, AND ACI 318-56 TYPE GIRDER SPECIMENS

Test beam No.	Type	A_s, sq in.		A_w,* sq in.		A_w/A_s		Ultimate load, kips		Writer's empirical load	
		Support	Midspan	Haunch	Total	Haunch	Total	Flexure	Axial	Percent	Kips
5	Wilkins	2.20	0.88	0.38	0.38	0.17	0.12	41.2			
9	Wilkins	2.20	0.88	0.38	0.38	0.17	0.12	17.7	19.3	31	19.5
10	Wilkins	2.20	0.88	0.38	0.38	0.17	0.12	21.7	15.2		
7	Robins	2.20	0.88	0.40	0.67	0.18	0.22	39.2			
8	Robins	2.20	0.88	0.40	0.67	0.18	0.22	30.9	28.4	33	20.8
4	1955 design	2.20	0.88	2.36	3.66	1.07	1.18	62.9		100	62.9
6	ACI 318-56	1.98	0.99	1.39	1.75	0.70	0.59	57.2		91	57.2

*"A_w Haunch" designates the area of the shear reinforcement from the support to the zero moment point; "A_w Total" designates the area of the shear reinforcement in half-span length.

0.60, and 0.18 percent was used in 13, 25, 25, and 37 percent length of Beam No. 4, resulting in shear steel quantities as indicated in Table A. The carrying capacity of the 1955 design cannot be inferred from the behavior of this specimen.

Beam No. 6 (representing ACI 318-56 design) contained nearly six times as much shear steel as the collapsed Wilkins girders; Beam No. 4 (1955 design) contained nearly ten times as much. These ratios depict not only the degree of deficiency of the collapsed girders in web reinforcement but also that of ACI 318-56.

Not only the shear reinforcement but also the length of the longitudinal steel was deficient in the Wilkins and Robins type girders. The application of axial tension to all specimens would have proved that if the longitudinal reinforcement is properly arranged, the normally present axial tension does not diminish the carrying capacity.

The statement on p. 637 that "Many similar structures (like the Wilkins girders) with integrally cast roof slabs . . . have an excellent service record" is misleading. Any girder with Wilkins type reinforcing has no definite factor of safety. The properly reinforced continuous girder has a reserve strength "owing to the redistribution of bending moments . . . no such reserve strength exists with regard to failure in diagonal tension."[7]

Recent investigations of Hajnal-Kónyi and Lewis in England and Sahlin and Nylander in Sweden, confirm that the maximum carrying capacity of the concrete girder can be assured only if failure in diagonal tension, shear, and bond slip are avoided.[7]

It is regrettable that specimens containing diagonal steel and evenly spaced small diameter stirrups were not tested. Diagonals hinder deformations effectively and take diagonal tension efficiently. Stirrups, when used in conjunction with bent bars, are much more efficient in hindering the enlargement of diagonal cracks, and the concrete hinders the elongation of the stirrups in inverse proportion to their diameter. Bent bars and small diameter stirrups constitute the superior arrangement as to strength, safety from large cracks, and cost. The steel quantities and the A_w/A_s ratios of the straight bar 1955 design and of a bent bar design are compared in Table B.

Costs

If the cost of the bent bars and stirrups is 1.2 and 1.7 times larger per pound than of the straight bars, the cost of the reinforcement for the bent bar design is 7.3 percent less than the cost of the steel in the straight bar design. This saving is equivalent to the cost of 42.4 lineal ft of #11 bar. This steel quantity can be built into the bent bar design to achieve equality of the cost of the designs.

All quantities in Table B refer to half-span length. The web reinforcement in the bent bar design in Table B is more than needed to cover all the shear by steel; nevertheless, this design contains less steel than the straight bar design with about 75 percent shear coverage. This indicates that no savings can be achieved by omitting web steel as compared to the risk taken.

A further measure for the "savings" achieved by omitting web steel is the estimate of cost of bomb proof structures by Neiman: "Structures designed for adequate resistance to blast loads (100 psf vertically down, 70 psf upward) are expected to have a reasonable chance of survival . . . The cost of construction that will provide this protection . . . was estimated to be 1 to 3 percent of the general construction cost of these buildings."[8]

TABLE B—STEEL QUANTITIES AND A_w/A_s RATIOS OF STRAIGHT BAR AND BENT BAR DESIGNS

Item		Straight bar design	Bent bar design
Top steel, lb		1357	Straight bars: 1092
Bottom steel, lb		658	Bent bars:
Haunch steel, lb		345	1253
Total main steel, lb		2360	2345
Closed stirrups, lb		422	151
Total steel, lb		2782	2496
Main steel area, A_s, sq in.	Support	21.84	
	Midspan	10.16	
Web steel area, A_w, sq in.	Haunch	16.40	29.04
	Total	26.40	45.02
A_w/A_s	Haunch	0.75	1.33
	Total	0.825	1.41

Another scale for these "savings" are the 20 rules of Amirikian.[9] Application of all these rules to all parts of buildings increases their blast resistance 100 percent for an increase in cost of 5 to 10 percent. One particular recommendation applies to this discussion: "Roof framing . . . (16) Concrete girders should have top and bottom face reinforcing continuous between supports, tied together with bands or stirrups through their span length."[9]

Strengthening existing structures

The problem of strengthening existing structures has frequently occurred in bridge design. A large number of railway girders and trusses were strengthened after World War I. Unlike in the United States, where the girders were usually doubled, new girders built for the other half of the bridge, and truss members strengthened by additional material, in Europe, girders and trusses were transformed into new statical systems by the addition of a third chord, usually in tension. The principles applied were identical with those of present-day prestressed construction.

Much experience has been gained concerning the behavior of girders strengthened by, or built with, a tension chord adjustable by means of jacks, screws, or shims, but there is no experience with the durability and permanency of action of 0.05 in. thick "stirrups" (straps) applied at the outside of large, haunched, concrete girders, the dimensions of which constantly change.

Arrangements by which loads on the girder can be carried to the columns are numerous. Such trussings can be simple if the introduction of axial compression into the girders is structurally feasible. Axial compression reduces the magnitude and changes the direction of the diagonal tension stresses and at the same time the magnitude and distribution of bending moments are advantageously affected. Examples of such arrangements are shown in Fig. B and C.

Fig. B shows the Saint Patrice Aqueduct in Spain, built in 1926.[11] In this structure, two 2.5-in. diameter cables at each side of the 318 ft long box girder were adjusted to the desired tension by hydraulic jacks placed on the

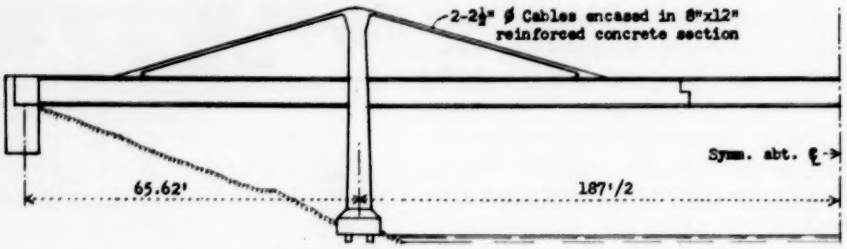

Fig. B—Saint Patrice Aqueduct, Spain, built in 1926

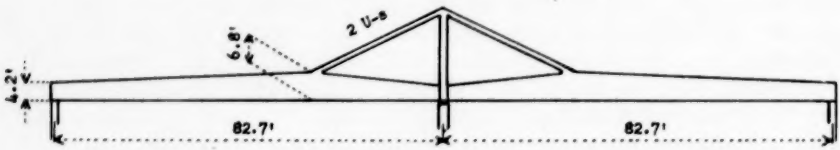

Fig. C—Precast concrete roof girders with structural steel trussing

columns above the piers and encased in 8 x 12 in. reinforced concrete. The application of rolled sections to precast concrete roof girders is shown in Fig. C.[12]

Similar arrangements are applicable for the strengthening of the Air Force warehouse girders, as indicated schematicly in Fig. D. If the introduction of compressive forces into the girders would be difficult, suitable schemes for the suspension of the girders can also be considered. Fig. E shows a scheme for this arrangement. These definite and durable arrangements of permanent action, easily adjustable if desired, are also economical.

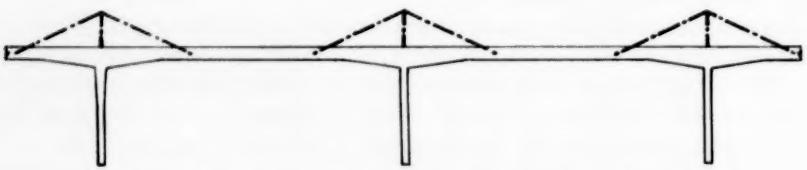

Fig. D—Scheme for strengthening existing girders by trussing

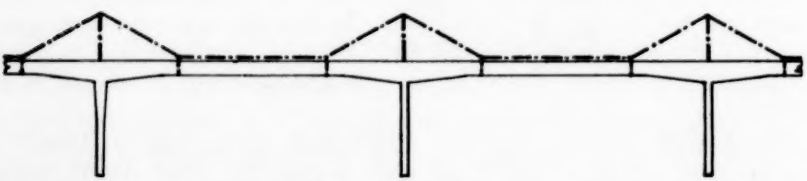

Fig. E—Scheme for strengthening existing girders by continuous tension chord

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By K. HAJNAL-KÓNYI*

The valuable research carried out by Elstner and Hognestad is open to various interpretations.

The failure of Beams 1 and 5 is consistent with the results of previous research referred to in "Laboratory Investigation of Rigid Frame Failure." However, as has been pointed out by the authors, it does not explain the Wilkins failure which was apparently caused by the simultaneous action of vertical and horizontal forces. The results of Beams 8-13 are therefore of particular interest.

Table C contains a summary of the axial tensile stresses f_t, vertical prestress f_v, shear stresses v, principal tensile stresses f_{pt}, and ratios $f_{pt}:f_c'$.

TABLE C—SUMMARY OF STRESSES

Beam No.	f_t, psi	f_v, psi	v, psi	f_{pt}, psi	$f_{pt}:f_c'$
8	338	0	113	372	0.0874
9	230	0	64.5	247	*0.0767*
10	181	0	79	218	*0.0737*
11	121.5	−117	225	257	0.0782
12	210	−117	90	233	*0.0733*
13	173	−711	219	291	0.0882

*Consulting Engineer, London, England.

In this table, tensile stresses are indicated as positive, compressive stresses as negative. The vertical prestress on the concrete has been assumed on the basis of an effective prestress in the steel of 46,800 psi (see p. 659):

$$f_v = -\frac{46{,}800 \times 0.75 \times 0.035 \times 2}{3 \times 7} = -117 \text{ psi.}$$

Some of the f_{pt} values, i.e., for Beams 8, 11 and 13, have no physical significance and have been included for the purpose of comparison only. The relevant figures are those for Beams 9, 10 and 12. Beams 9 and 10 may be considered as completely unreinforced for the forces acting upon them, since the bottom bars were entirely inefficient for the combination of vertical and horizontal tensile forces. In the same way, Beam 12, although provided with straps, may be considered as "unreinforced" for carrying an axial tensile force of such magnitude that the force in the "compression" zone became tension.

It should be noted that the ratio $f_{pt}:f_c'$ for these three beams varies within the narrow range of 0.0733 to 0.0757. It may be sheer coincidence but should nevertheless be mentioned that these values are only slightly lower than the ratio of the tensile strength of concrete to the cylinder compressive strength according to Carneiro in Fig. 3 of the paper by Thaulow, in the same issue of the JOURNAL.[1]

It should be realized that the values of f_t and v in Table C are averages and that these stresses are not uniformly distributed over the whole cross section, e.g., the maximum shear stress in a homogeneous rectangular section is 1.5 times the average and is greater than the average related to $\frac{7}{8}$ times the effective depth of the cracked section. Consequently, the f_{pt} values given in the table must have been exceeded locally. It is therefore probable that maximum $f_{pt}:f_c'$ was at least as great as obtained from Carneiro's curve.

The failure of Beams 9, 10, and 12 may therefore be explained as that of an unreinforced concrete beam in which the principal tensile stress has reached the tensile strength of the concrete. Elstner and Hognestad have called this "separation" failure which, if the foregoing considerations were correct, would apply not only to Beams 9 and 12 but also to Beam 10. Indeed, a comparison of Beams 9 and 10 after failure as shown in Fig. 6, p. 645, suggests a similar type of failure in both cases.

The principal tensile stress cannot, of course, be a criterion of failure if sufficient reinforcement is available to take up the tensile stresses when the tensile strength of the concrete has been exhausted. This applies to Beams 8, 11, and 13 for which the ratios are greater than for Beams 9, 10, and 12. From the figures given by the authors on p. 651, it seems probable that in Beam 8 yielding of the longitudinal reinforcement was the primary cause of the failure. In the case of Beams 11 and 13 the axial force was kept so low in relation to the bending moment that the compression zone remained under compression and the straps could cope with the shear stresses until the yield point was reached or exceeded both at the supports and in the span.

The authors have raised the question (p. 650) whether failure is probable for an axial tension of 100 psi due to shrinkage and temperature drop at a calculated flexural shearing stress of about 110 psi. With $f_t = 100$ psi, $v = 110$ psi, $f_v = 0$, $f_{pt} = 171$ psi. With $f_c' = 2520$ psi (the lowest value in Table 1, p. 640), $f_{pt}:f_c' = 0.0678$. On the basis of the preceding considerations, the answer is therefore that failure would not be probable, but the margin of safety would be rather small.

Nevertheless, the writer is of the opinion that even the design by ACI 318-56 as represented by Beam 6 (Fig. 8, p. 654) is bad practice and would not be acceptable in Europe to any experienced designer, neither in rectangular nor in T-beams. In the case of the latter, it is considered to be of vital importance to connect the web with the flange.

The elastic analysis is only a guide for the designer. In a structure of such dimensions as the Wilkins depot, allowance must be made for unforeseen and unforseeable circumstances such as uneven settlements. It is not suggested that such settlements had anything to do with the failure, but their possibility must be taken into account in the design. It is therefore necessary to provide nominal links and lapped top reinforcements over the whole length of the frame. According to the writer's view, a "nominal" reinforcement must be strong enough to take over the tensile stresses from the concrete in case of cracking. A reinforcement of 0.08 percent of structural grade quality as mentioned on p. 643 is tantamount to no reinforcement at all. The stirrups provided in the 1954 design represent only 0.06 percent of the web and it should have been a foregone conclusion that they would not substantially influence the load bearing capacity of the frames, as proved by Beam 7.

It is unlikely that the yield point of the longitudinal reinforcement would have been reached in Beams 11 and 13 if the reinforcement of these test beams had been of the same quality as in the prototype frames, i.e., if it had had a yield point of over 100,000 psi instead of an average of about 50,000 psi. The aim of good design must be a factor of safety which is as nearly uniform as possible. This can hardly be realized in practice, but what can be achieved is the prevention of brittle failure, such as characterized by Fig. 13(b). The provision of an excessive amount of reinforcement at places where it can never develop its strength is of no use, particularly if at the same time vulnerable parts of the structure remain completely unreinforced.

The writer is fully aware of the fact that conditions in the United States are different from those in Europe and that the main consideration in the United States is saving in labor and not saving in materials. Nevertheless, this tendency must not go so far as to jeopardize the safety of structures.

From the point of view of European practice it is impossible to understand the design philosophy of the frames in question. On the one hand, the use of a working stress of only 20,000 psi for steel of a yield point of over 100,000 psi seems to be extremely wasteful; on the other hand, the curtailing of bars strictly in accordance with a bending moment diagram obtained from an elastic analysis without any allowance for possible shifts of the point of contra-

flexure is a serious error on the unsafe side. This was aggravated by the sudden ending of seven bars of 1⅛ in. diameter at the same section, and by the absence of stirrups, as confirmed by the mode of failure of Beams 1 and 5, in which the point of contraflexure was at its theoretical position.

There is no doubt that with the same total, or even smaller, quantity of steel as used in the 1952 design, frames with a satisfactory performance could have been constructed if the working stress had been raised to say 35,000 psi and the balance of the steel distributed according to the principles outlined in this discussion.

In conclusion, it is pertinent to quote from the report by ACI Committee 315 in the same issue of the JOURNAL:[2]

"Actual structures may not behave exactly in accordance with the design assumptions, which necessarily idealize them a bit, so allowances must be made for possible variations. The most carefully prepared design can be spoiled by failure to reinforce a critical point properly. There is a great deal more to making a satisfactory design than the mere computing of maximum positive and negative flexural reinforcement. The real skill comes in developing satisfactory details."

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By ERNEST H. HARDER*

The study of a structural failure is often more satisfying than the study of a success because one can learn about precautions for the future. In this particular failure, this writer has contended from the first publication in *Civil Engineering* (V. 26, No. 2, Feb. 1956) and in *Engineering News-Record* (Jan. 12, 1956) that the selection of this design was improper. It is a typical structural steel warehouse design with long main girders and shorter and lighter purlin spans—but without continuity. In this design, there was the element of continuity which was the real basic cause that led to collapse.

A study of the general plan of the special AMC warehouse, Wilkins Air Force Depot, Shelby, Ohio, shows that this huge building was divided in half by a long east and west expansion joint a little to one side of column row No. 10. In the north and south direction, the building was divided by expansion joints into ten bays so that there are 20 building units each approximately 200 x 200 ft square. In the north and south direction are the long span girders which failed and it is these failures which this writer will attempt to analyze.

Unfortunately the test results from the one-third scale model cannot be correlated with the failure of the prototype; in fact, they can be correlated only with an earnest endeavor to break the model by applying a sufficient

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vertical structural load. A certain satisfaction was gleaned by the fact that the crack which caused the failure at Wilkins was closely reproduced in the laboratory (Beam 10). But why was it necessary to make an expensive model to find that the break came near the point of contraflexure? Where else could it have failed? There is no mystery about this point being the location for a shear failure.

Elstner and Hognestad declare that "the fundamental mechanism by which the collapse at Wilkins took place is, as yet, not entirely clear." Since I was interested in this problem, I first of all made a careful elastic analysis to try to discover the underlying causes of the failure. In this analysis, I took cognizance of the shape of the girders and columns; but because of meager information, I assumed that the columns were fixed at the bottom instead of being hinged. From available information, I computed the dead load under which collapse took place to be 2.18 kips per ft of the long girders. Later a figure of 2.33 kips per ft was published, which is but 7 percent higher than the figure of my choice. Due consideration was given to the correction for sidesway and for a free sliding versus a frozen expansion joint. For dead load it made little or no difference whether the joint was frozen or free because the correction for sidesway is small. The only condition which could be affected by a frozen joint was in the matter of temperature changes and in shrinkage.

In my analysis, I concentrated on that span in Bay 6, Frame 1, between Columns 14 and 15. The temperature change was assumed to be ± 50 F with $E = 2 \times 10^6$, $\epsilon = 6 \times 10^{-6}$, and coefficient of shrinkage = 0.0002. I further chose a section 20 ft from Column 15, that section being about at the center of the diagonal crack.

At this section, the unit dead load shearing stress was 46 psi. For a temperature change of ± 50 F the unit shearing stress was 4 psi, while shrinkage caused a unit stress of 3 psi for a free sliding expansion joint. For a frozen joint the 50 deg temperature change caused a unit shear of 8 psi and 5 psi for shrinkage. It is difficult to see how these low unit shearing stresses could have caused the original collapse. The question of direct stress has been introduced into the test. In the span involved, the dead load caused a direct compression of 14.2 kips across any cross section. For 50 deg temperature change, the direct stress was 8.95 kips and 6.0 kips for shrinkage and a free sliding joint. With a frozen expansion joint, the theoretical direct stress was 27.3 kips for the temperature change with a probable 18.2 kips tension for shrinkage. The possible bending stresses at the chosen section were small.

A few comparisons between stress conditions in the model and those in the prototype from Table 1, p. 640, show that for Beams 1 and 2 the total vertical flexural load was 38.7 and 41.7 kips, respectively, with shearing stresses of 141 and 163 psi. But the total dead load of the prototype at the time of collapse was 145 kips with a unit shear of 46 psi. In the model, the comparable load would have been one-ninth of 145 kips or 16.1 kips with a unit shear of 46 psi. In Beams 9 and 10, the vertical flexural load at the time of the collapse of the model was, respectively, 17.7 and 21.7 kips with 19.3 and 15.2

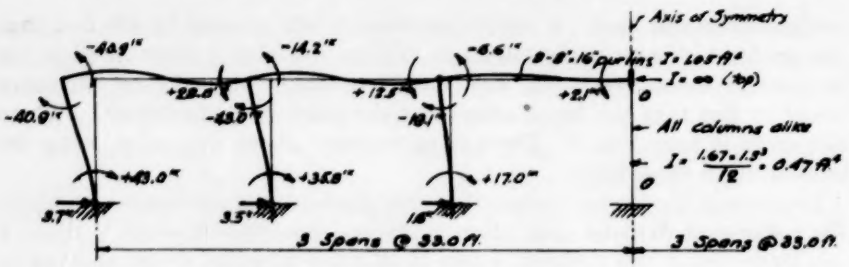

Fig. F—Analysis of frame

kips axial tension in the model. But for the dead load of the prototype, there was an axial compression of only 14.2 kips. Something doesn't correlate.

While the design seems to have been sufficiently adequate in the long direction for 200 ft, the adequacy of the 200 ft at right angles was neglected. Here we find for each 66-ft span, eight purlins made of concrete block prestressed by internal cables. In describing the original structure, except for a brief mention that continuity of purlins over main girders had been provided, no details were shown. It seems that there was an overconfidence that the purlins had nothing to do with the collapse; there was merely some concern with shear at a point of contraflexure. But no one pointed out that the 20 x 36-in. main girders were in violation of Section 704 of the 1956 and previous ACI Codes. Perhaps too much reliance was placed in the appraisal of the rigidity of the purlins.

The fact remains that for months, with temperature changes only, the purlins kept pushing and pulling and twisting the long supporting girders until they could resist no longer. Bent 1, Bay 6, at Shelby was cast Feb. 10, 1954; cracked Aug. 3, 1955; and collapsed Aug. 17, 1955. Herein lies that "fundamental mechanism" which contributed to the collapse.

To try to evaluate this action, I assumed a six-span frame with 33 ft between 18 x 20-in. columns. The horizontal beams of this frame are eight 8 x 16-in. purlins. Since the dead load transmitted to the long girders would not exert any torsion except at the end girder support, I concentrated on temperature changes. Shrinkage is out of the question since the block purlins were precast. Here we have a perfectly valid frame which can be reasonably analyzed by moment distribution or by any other method. The results are given in Fig. F. Here at the girder which first failed and which I have used throughout this discussion, we have at the column, a torsional moment of 43.0 ft-kips.

Because of the system I have used, this torsion is distributed more or less uniformly throughout the 66-ft girder to the next adjacent columns. The shearing stress of a rectangular section in torsion is given by $\tau_{max} = M_t/\alpha bc^2$ where $M_t = 43{,}000 \times 12$, $b = 36$ in., $c = 20$ in., and α is a numerical factor which depends upon the ratio b/c, in this case 0.240; τ is 150 psi. Add this to the dead load shear of 46 psi in the main girder and 4 psi for 50 deg temperature change, we have the sizable total unit shear of 200 psi at the section 20 ft from Column 15.

By JOSEPH E. PERRY*

Messrs. Lunoe and Willis, in "Application of Steel Strap Reinforcement to Girders of Rigid Frames, Special AMC Warehouses," have accurately presented the methods and procedures adopted for strengthening the girders of the rigid frames for the special AMC warehouses. The test application of steel straps during December, 1955, proved most helpful to those faced with the task of developing plans and specifications for strengthening girders in other warehouses.

Thirteen rigid frames of Warehouse A, Tinker Air Force Base, Oklahoma, required strengthening; the remaining seven rigid frames were constructed in accordance with revisions prepared by Ammann and Whitney for the Office, Chief of Engineers. Application of straps in Warehouse A was accomplished in the manner described by Lunoe and Willis. Modification of the Signode Model W-2 stretcher varied from that described. The nut fitting the torque wrench was welded directly over the center of the drive shaft of the gear box.

The essential difference between the two projects was in the determination of the strap stress. Each tensioning unit (stretcher and torque wrench) was calibrated in a commercial laboratory. After laboratory calibration, a field check was made to verify the laboratory work by tensioning and sealing straps on a girder (tapping the straps during tensioning and after sealing). Then by use of SR-4 electric strain gages, two Dumont strain gage controls, and a multi-channel Sanford recorder (Model #150), the stress was determined. Allowances were made for friction and other field conditions. Upon completion of calibration, actual field application was begun. Approximately 10 percent of the straps were stress checked by using a 10-in. Whittemore extensiometer. Two holes were drilled in each vertical leg of the in-place-strap and the distance between holes was gaged. The strap was cut from the girder and the holes were regaged. The differential between gagings was converted to stress. In addition to the mechanical stress determination, accuracy of each set of tools was checked once a week by use of electric gages on in-place-straps. Before work each morning and noon the torque wrenches were checked by use of a simple cantilever and weight system. One crew was organized and started work. When the initial crew appeared sufficiently trained they served as crew leaders as new crews were formed.

The increment method for tightening the straps described by Lunoe and Willis was used during crew training; however, after crews became proficient and understood the reasons for incremental tightening and tapping they were allowed to tension the straps in one continuous operation, tapping the strap simultaneously. Results of the two procedures were compared and found to be practically identical.

The writer concurs with the conclusions drawn by Lunoe and Willis.

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By J. R. SHANK*

I read with much interest the article "Rigid Frame Failures," by Anderson, and I want to commend him and the firm Ammann and Whitney for the fine work they have done in analyzing the failure of the rigid frames at the Wilkins Air Force Depot, Shelby, Ohio. The profession would be farther along if all failures in the past had been so adequately studied and published.

My interest is heightened because of having had occasion to inspect this failure soon after it occurred and to do some work on it.

My inspection revealed two things that were of particular interest to me. (1) The failure occurred at or near the termination of the top steel, all of which was terminated at one beam cross section. (2) An inspection of one of the expansion joints of the failed frame showed that it had never operated. Mortar or grout that had run down over the edges of the top and bottom plates of the joint during construction was hardened and in place showing no cracks at the horizontal expansion plane.

It is not uncommon for engineering failures to have occurred because of bad details rather than of bad major design features. I am of the opinion that this failure was due largely to bad details. All reports say that the major design and construction was of the best modern engineering practice.

A designer who is thoroughly acquainted with materials and knows the effect of various details, as well as the best major design procedures, will avoid sudden changes of cross section and avoid re-entrant angles as he would gross errors in design. The designer of iron or steel castings is careful to place as large a fillet as conditions permit where a re-entrant angle is unavoidable. Structural welding procedure will not permit a plate girder cover plate to be cut off square and welded across the end. It must be scarfed and welded along the converging side edges.

Concrete does not adhere to steel, bond is mechanical only. The presence of steel running at right angles to a stress in a cross section must be considered as so much void in the cross section of concrete. Simple beams tested under concentrated loading develop cracks during the test at the stirrups, revealing their location. The termination of concrete reinforcing steel is particularly bad because it is placed as close to an edge as protection demands will permit and cracks are most likely to start at a boundary or edge.

I would like to present an hypothesis of what may have happened.

In the early life of the beam, while still in the forms, when the concrete was setting and shrinking and little relief was had at either the expansion joint or the end column, the tendency to form cracks was invited to concentrate at the weak point at the top bar terminals instead of being distributed along the beam as would have been the case if top steel had continued along the top. It is possible that the cracks may have completely crossed the beam at this time when the bottom steel would offer little resistance because of the weak

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condition of the concrete, or it may have merely started at the bar terminals and finished the crossing after the form supports had been removed. This latter possibility is indicated by the inclination of the crack.

Normally, diagonal tension failure has a high hurdle to cross at the tension steel, but here the crack came from the other side as a result of the shrinkage. It is hard to imagine any effective diagonal tension resistance in the concrete under the conditions mentioned above. It seems to be unfair to diagonal tension resistance to blame the failure on it alone.

The casting or welded fabrication designer speaks of making a clean design by avoiding stress raising details such as abrupt changes of cross section, re-entrant angles, etc. Concrete designers may do well to take some lessons from them.

It is hard for a code writer to prohibit bad details; only good engineering sense can take care of this.

Closure by BOYD G. ANDERSON

The discussions of Dr. Abeles and Mr. Balog consider the amount of web and flexural steel necessary to secure adequate shear strength in reinforced concrete members, the problem leading to the ACI 318-56 code modification. Mr. Shank and Mr. Balog are also concerned with the proper detailing of reinforced concrete members.

Mr. Balog expresses the opinion that the German practice is the only logical solution to the shear problem. The German practice assumes that after an allowable load based on the elastic tensile strength of concrete is exceeded at any point along the length of the member the web steel must be designed to carry the full shear for the entire shear span (half length). Mr. Balog defends this practice by stating that the increase in cost of incorporating this design concept will be small.

The writer in his article covered many of the points discussed by Mr. Balog and stated that of the existing design codes, use of the German code would apparently have resulted in a design that would have prevented the warehouse failure. However while use of the German code would increase the total cost of most buildings by a relatively small amount, it would increase the cost of beams and girders in the structural portion of the work by a substantial amount and would place reinforced concrete at a competitive disadvantage that should be accepted only if justified by necessity. Furthermore there seems no assurance that designing web steel for full shear would necessarily prevent failure in members having high axial tension and fairly widely spaced vertical stirrups. Such stresses can occur in continuous structures having locked-in stresses.

A good deal of evidence indicates that properly behaving structures may depend more on sounder knowledge of the stress transfer possibilities of reinforced concrete members and on better analytical methods. The laboratory tests of the rigid frame failure showed that less web steel than required by the

German code would have developed the flexural strength of the frame. The simple beam tests have indicated time and again that the concrete can carry or assist the web steel in carrying shear after cracks appear and have even suggested the possibility that the load capacity of such members may be represented solely by the flexural capacity, the steel anchorage details, and the maximum allowable crack widths. Adequate steel anchorage and detailing procedures such as suggested by Mr. Balog and Mr. Shank, are a prime requisite to acceptable design.

Whitney's recent JOURNAL paper* studies the relationship of flexure and shear strength, posing simple principles that appear to offer an excellent correlation with test data. His paper suggests much in the way of future testing and study. Amplifying this paper the author would like to call attention to the fact that so-called diagonal tension failures for a large number of different beam depth to span ratios picked at random showed a large scatter in conventionally computed shear stress at failure but an orderly and limited range of data when compared to flexural strength and the end bond stress in the reinforcing steel over the support at the time of failure. This bond stress incidentally compared well with the bond stress for smooth and high-bond bars obtained in pull-out tests.

Dr. Abeles' thoughts regarding bond slippage and the effects of axial or flexural tension or compression in the members amplify the author's stated opinion that these factors appear to be a nub of the shear design problem. Whitney's shear equations, for example, recognize the effect of axial stress on the shear capacity.* Precompression by prestressing of under-reinforced members automatically increases the shear strength in his equations.

The comments of Mr. Balog and Dr. Abeles regarding the condition at the point of contraflexure are particularly pertinent. While a number of mechanisms appear available to carry the shear stress with minor acceptable cracking in simple beams and in other parts of continuous or restrained members during the course of the loading, the strength at the point of contraflexure depends on the tensile strength of the concrete unless other stress transfer means are introduced into the member. Accordingly this area should contain web reinforcement adequate to carry a larger part or all of the shear load occurring in that region. Negative and positive steel should anchor well beyond the point of contraflexure preferably with a good portion of this reinforcement bent as truss bars in the area of contraflexure.

Mr. Balog also suggests a number of methods by which distressed structures may be reinforced. Similar measures were considered for the warehouses but were abandoned because of clearance problems, the unfeasibility of opening the roof membrane over active stored areas, and because of the difficulty in applying a known prestress force to a frame integrated or connected to roof framing with restraining shear walls.

*Whitney, Charles S., "Ultimate Shear Strength of Reinforced Concrete Flat Slabs, Footings, Beams, and Frame Members Without Shear Reinforcement," ACI JOURNAL, Oct. 1957, *Proc.* V. 54, pp. 265-298.

Closure by RICHARD C. ELSTNER and EIVIND HOGNESTAD

The discussions by Messrs. Balog, Hajnal-Kónyi, and Harder are concerned mainly with three subjects: analysis of the cause of failure in the prototype; interpretation of test results; and philosophy of reinforced concrete design. Since only the first two subjects are directly pertinent to the laboratory investigation, discussions concerning "full" shear reinforcement, bent-up bars, European specifications, and economics will be omitted in this closure, although the authors appreciate the views expressed on these important aspects of reinforced concrete design.

In analyzing the cause of failure in the prototype, Mr. Harder showed that the computed axial stresses introduced into the prototype frame by reasonably assumed temperature change and shrinkage were less than the axial stresses required to cause failure of the laboratory test specimens. Ruling out axial tension, then, Mr. Harder considered the hypothesis that the warehouse failures were caused by a combination of shear and torsion. In the author's view, however, observed facts eliminate torsion as a primary cause of failure.

(1) Mr. Harder's analysis shows the calculated tensile stress from torsion to be three times as large as the tensile stress due to shear. Therefore, one should expect typical torsional characteristics of the failure. The actual collapse, as shown by Fig. 6 in Anderson's paper[1] does not resemble a torsional failure especially when compared to the many torsional failures in reinforced concrete beams illustrated by the laboratory tests of Bach and Graf.[2] In the collapsed girder of the prototype, the surface of failure was a diagonal plane, not a helical or twisted surface, as would be the case in a torsional failure. However, the slope of the diagonal plane was somewhat greater than is usually observed in shear failures, thus indicating the possibility of combined axial tension and shear.

(2) Mr. Harder's analysis indicates torsion to be a maximum for the end frames (Frame 1 and Frame 5). Because of symmetry, the center frame (Frame 3) would not be affected by torsion. Therefore, although the torsion hypothesis might be applicable to the collapse of Frame 1 at Wilkins Air Force Base, it does not explain the simultaneous collapse of the adjacent Frames 2 and 3; nor does it explain the failures at Warner-Robins Air Force Base[3] of Frames 2 and 3 while Frame 1 remained intact. Likewise, difficulty is encountered in explaining by torsion the distress of similar warehouses which had roofs of precast channel slabs without purlins. The greatest difficulty comes in explaining by torsion progressive cracking observed in free-standing frames.

A torsional analysis does not agree with field observations, and the laboratory tests indicate that shear alone cannot logically explain the failure of the prototype. In spite of the mathematical elastic analysis, the detailed observed information concerning the failure could not rule out the possibility of axial tension being a primary factor. Mathematical analyses are valuable engineering tools, but their applicability is in doubt when they yield results contradicting observed phenomena.

Dr. Hajnal-Kónyi has interpreted part of the test data in terms of principal tensile stresses. For Beams 9, 10, and 12 the principal tensile stresses at the point of contraflexure approached the ultimate tensile strength of concrete, with values ranging from 0.0733 f_c' to 0.0757 f_c'. However, Beams 1 and 5 were identical to these three beams except for the omission of axial tension.

Their principal tensile stresses were 0.0381 f_c' and 0.0522 f_c' respectively. These values do not approach the ultimate strength of concrete as determined by tensile tests.

The possibility that the tensile strength is influenced by the presence of an orthogonal compressive stress cannot be ignored. The addition of a uniform axial tension reduces the principal compressive stress at the point of contraflexure. In Beams 9, 10, and 12 the principal compressive stresses were small, 17, 36, and 139 psi, respectively. As the principal compression approaches zero, the condition of a pure tension test is approached. However, with no superimposed axial tension, the principal compression at the point of contraflexure is equal to the principal tension. The tests seem to verify other data which indicate that an increasing principal compression reduces the ultimate tensile strength. This hypothesis warrants further investigation.

Mr. Balog presents an empirical equation which states that a beam can develop full capacity only when A_w/A_s is equal to unity. If we define A_s as the tensile steel area at a point of maximum moment (positive or negative) and A_w as the total shear reinforcement area from a point of maximum moment to a neighboring point of zero moment, then it can be shown mathematically that for any beam loading condition, support condition, and degree of fixity, A_w/A_s is equal to the calculated shear carried by the web reinforcement divided by the total applied shear. When all shear is carried by web reinforcement, A_w/A_s is equal to unity.

Mr. Balog has incorporated A_w/A_s into a parabolic equation which requires A_w/A_s = unity, that is, full shear reinforcement, to develop full load carrying capacity of a beam or frame. Mr. Balog has applied this equation only for the region from the support to the point of contraflexure.

The region from the point of contraflexure to the point of maximum positive moment should also be considered similarly.

This laboratory investigation and the discussions point out the latitude of existing opinion regarding shearing strength of reinforced concrete. The authors and discussors agree that the load capacity of a concrete girder can be assured only when failure in shear is avoided. Shear failures can be prevented by sufficient web reinforcement, but the dividing line between structurally sufficient and economically wasteful web reinforcement remains to be determined.

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3. *Engineering News-Record*, Sept. 27, 1956, p. 28.

Discussion of a paper by Wilhelm Eitel:

Recent Investigations of the System Lime-Alumina-Calcium Sulfate-Water and Its Importance in Building Research Problems*

By M. SPINDEL, HAROLD H. STEINOUR, and AUTHOR

By M. SPINDEL†

The author has dealt with studies of the effect of complex calcium aluminate sulfate hydrates on the deterioration of mortar and concrete, dating from W. Michaelis' discovery of his so-called *Zement-Bazillus* in 1892 up to the latest highly scientific investigations in the United States.

The details given are of great importance to all who have to protect mortar and concrete from aggressive sulfate water, wherever it occurs in the building industry. Since the beginning of this century many investigations on this matter have been made both in the laboratories of research stations and by numerous committees in the field. The main result obtained was that the only nearly adequate protection against sulfates, as against some other aggressive salts, was to make the concrete completely waterproof, i.e., completely impervious to water at least on the surfaces where the structure could be contacted by the aggressive water.

Of course, such waterproof concrete has to be made with a minimum of cement and with the minimum water-cement ratio and effective density, utilizing compaction or vibration in placing the concrete. Often the best results are obtained by using some admixtures. The most effective, according to latest developments, are surface active agents combining dispersion with air entrainment. Steaming under high pressure is helpful too.

The writer arrived at the above conclusions as head of a material research station and as a consultant on many jobs where the bad effect of sulfate waters had to be eliminated. It would be of interest to know whether the author agrees to the above measures in principle, and any further practical advice would be appreciated by all concerned.

The paper deals with many more influences on the deterioration of mortar and concrete than one usually expects to occur, for example, that some pozzolans may prove harmful instead of useful as expected, so that additional precautions would be necessary; but it is important to know wherefrom the attack may come.

The space available for discussion would hardly allow one to go into the many details of this important paper. One question may be allowed. Why

*ACI JOURNAL, Jan. 1957, *Proc.* V. 53, p. 679. Disc. 53-37 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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does the author not make use of the fourmatter-parallelogram which has been presented by the writer at several international congresses and in many publications since 1927?*

Some well-known chemical engineers agreed about the advantages of this parallelogram, which is the most correct projection of the mathematical representation of four matters in space and can be extended even to five matters, giving the exact percentages of each matter and of combinations of two or three of them, the total always being 100 percent. Prof. H. Nitzsche, cited by the author (Reference 2), admitted that this fourmatter-parallelogram allowed him to deal easily with 13 different materials for making one cement of them. It would be of interest if the author would consider this too.

By HAROLD H. STEINOUR†

Dr. Eitel's fine review and discussion deals with developments that I have been following with interest in recent years. As is clearly shown, these studies have important significance for concrete science. So concise a presentation cannot, however, go into all details. Since Dr. Eitel has been concerned primarily with application to aggressive sulfate solutions, I venture to add a little to what is said regarding application to the course of hydration of portland cement.

The system CaO-Al_2O_3-$CaSO_4$-H_2O does not include all the oxides of portland cement that might conceivably affect the solubility relations and the solids that precipitate. For this and other reasons, there are some limitations to the confidence with which one can make application to the hydration of portland cement. However, like many other products of the laboratory, these studies constitute a tentative guide, which may be modified as further knowledge is gained.

With this understanding, Dr. Eitel's Fig. 10 enables one to follow the apparent course of hydration of portland cement insofar as its content of calcium aluminate is concerned. Indeed, Dr. Eitel has greatly facilitated this and other applications by his choice of logarithmic scales which make the regions of crystallization of the various solids stand out more clearly. He has not, himself, made detailed application to the hydration of portland cement. In somewhat greater detail, one may state the deductions from Fig. 10 as follows.

Since portland cement saturates the mix water with calcium hydroxide and gypsum and must precipitate some kind of hydrate containing alumina, the early solution composition must be that at point *F*—for *F* is the only point where the indicated solids coexist. The new solid containing alumina is thus seen to be ettringite ($3CaO \cdot Al_2O_3 \cdot 3CaSO_4 \cdot 32H_2O$). It forms at the expense of the gypsum, which therefore eventually disappears. When that happens, the further course of the same reaction causes the solution composition to lose $CaSO_4$ and move down along boundary *FGd*.

*Spindel, M., Question III, Special Cements, Second Congress on Large Dams, Washington, D. C., 1936. See also writer's discussions in ACI JOURNAL: Nov. 1943 Supplement (*Proc.* V. 39) and Dec. Part 2 1950 (*Proc.* V. 46).

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According to Fig. 9, a new solid could appear at *G*. Under the given conditions, however, it does not form readily, and Fig. 10 assumes that it does not form at all. Instead, the precipitation of ettringite continues to point *d*, where a new (metastable) solid begins to form, which is identified as a crystalline solution. This is a solid solution of calcium aluminate hydrate and the low-sulfate double salt: $3CaO \cdot Al_2O_3 \cdot CaSO_4 \cdot 12H_2O$. At *d*, the composition of the solid solution is approximately that of this sulfate end member, as may be seen from Table 2.

This new solid requires sulfate, for which the only source is now the ettringite. Thus, at *d*, the calcium aluminate still coming into solution from unreacted cement reacts with ettringite to form $3CaO \cdot Al_2O_3 \cdot CaSO_4 \cdot 12H_2O$.

As Dr. Eitel indicates, the ratio of Al_2O_3 to SO_3 is higher in most portland cements than in this low-sulfate double salt. In such cements, the ettringite disappears at point *d*, and the liquid composition moves down along *dv*. Thus, the ratio of Al_2O_3 to SO_3 in the crystalline solution increases.

This, then, is the early course of hydration of portland cement, if it conforms to Fig. 10. It will be noted, however, that one solid that is only metastable is assumed to form in preference to the stable one. Conceivably, other departures from the stable relationships may also occur. Some believe, for example, that the low-sulfate double salt or its crystalline solution may precipitate initially or along with the ettringite. Also, the *delayed* rapid reaction observed by Lerch* is tentatively explained by him as rapid crystallization of calcium aluminate hydrate. If he is correct in this, the dissolution of ettringite at point *d*, or the subsequent adjustment of the crystalline solution must be too slow to accommodate all the calcium aluminate coming into solution.

Thus, solubility relationships like those of Fig. 10 do not always show what the course of reaction actually is, but they do show what *tends* to occur, in cases that the investigated systems sufficiently represent. This basis for judgment, though not as certain as death and taxes, is still useful.

AUTHOR'S CLOSURE

HYDRATION PHENOMENA

It is highly appreciated what the discussion of Dr. Steinour contributes to the importance of D'Ans' and Eick's investigations as related to the hydration phenomena of portland cements. This field of application of those data was not demonstrated to any great extent by the author because he wished to concentrate on the field of the reactions of sulfates with pozzolanic and related materials used in concrete. It is, on the other hand, particularly true that nonequilibria play an important role in both fields of phenomena, as indicated by Fig. 9 and 10, schematically, for the extreme cases of complete equilibria, and high degrees of nonequilibria in the special case of the alumina hydrate gels. The nonequilibria are made evident in a series of

*Lerch, William, "Influence of Gypsum on Hydration and Properties of Portland Cement Pastes," *Proceedings*, ASTM, V. 46, 1946, p. 1252; also *Research Bulletin* No. 12, Portland Cement Association.

degrees of "aging," with corresponding different degrees of reactivity with solutions. The static diagrams as they are given do not, of course, indicate anything on the kinetics of the crystallizations, e.g., the possibility of a rapid nucleation and precipitation of a calcium aluminate hydrate as discussed by Lærch in the paper cited in Dr. Steinour's discussion.

SULFATE REACTIONS

It is a pleasure to see that the author was well understood in his desire to interest civil engineers and chemists in the problems of sulfate water reactions on activated silicate systems, whether these be the usual cement-bonded concretes or pozzolanic material of a most diversified chemical constitution. It is not possible to give here a general answer to the problem of how sulfate deterioration can be safely eliminated. There are too many special conditions to be considered in every practical case of a pozzolan occurrence and its behavior with hydraulic binders. But one fact is certain; it would be a great and responsible error if one thought that the problems here in question are solved exclusively by the knowledge of the existence of expansive calcium sulfate aluminate hydrates. D'Ans' and Eick's investigations give the basis for a quantitative treatment of the sulfate reactions, leading to an understanding of why in one or the other case the formation of the complex salts may become dangerous for the existence of a concrete structure, and can give an estimate of safety of measures intended to overcome sulfate reactions.

To make quantitative considerations possible, and in the form in which physical-chemical equilibrium studies are submitted to the engineer, the investigators commonly give the projection of their results by an "equilibrium diagram." The usefulness of such a projection may be compared to that of a geographical or a geological map in the hand of the civil engineer, or to the alloy phase diagrams from which the metallurgist must try to develop the best possible properties of a modern steel composition. It is admitted that some experience is necessary to fully understand the importance of such a complex system as that which is discussed in our case. It is a so-called polynary system, i.e., a system with many components, for which special methods of two- or three-dimensional projective geometry must be applied. This major difficulty can be overcome by some study of the references presented in the following, giving particular appreciation to Mr. Spindel's highly encouraging remarks.

When W. Gibbs developed the fundamental rules of thermodynamics for phase equilibria in physical-chemical systems he did not restrict his calculations to any simple systems of only two or three components. His derivations are equally valid for "simple" and polynary systems. The language of his general equation systems based on the fundamental concept of the thermodynamic potential is, however, not that which the engineer will usually understand. Therefore many authors after Gibbs gave elementary introductions for the use of binary, ternary, and even quaternary melt equilibria, or salt crystallization equilibria. J. E. Ricci[1] gives the student a modern and

excellent introduction to the kind of extremely useful equilibria diagrams used in chemistry and metallurgy. For the cement chemist, a fundamental projection of the equilibria in the simplified system CaO-Al_2O_3-SiO_2 was given by G. A. Rankin[2] which illustrates the reactions occurring in the formation of "white" portland cement clinkers. The enormous advance which this diagram has given to accurate knowledge on cement reactions is generally known. It has influenced greatly the development of the best possible qualities of cement, and showed ways to avoid its detrimental instability by dusting, free lime hydration, etc. The modern cement specialist, however, knows that the Rankin system is only a first approximation to the real industrial problems of making cement, not from chemically pure ingredients, but from more or less "contaminated" natural raw materials. The three-dimensional projection of the system CaO-Al_2O_3-SiO_2 is no longer sufficient if the important effects of Fe_2O_3 and of alkalies, MgO, etc., on the reactions in the formation of cement clinker and the technological properties of a real "gray" cement are concerned. The cement chemist was compelled to study polynary systems, especially the highly complex system CaO-Al_2O_3-Fe_2O_3-MgO-K_2O-Na_2O-SiO_2.

The theory of treating such polynary systems by static investigations and to evaluate the results by methods of projection is by no means a hopeless enterprise. In a strict analogy with the usual three-dimensional projection in "space," viz., by an end view, side view, top view, it is possible to use general projective methods of a polydimensional geometry as developed by the mathematician Schoute.[3]

The methods of Schoute are not easily read. In principle they are useful, and have been discussed in a similar treatment of physical-chemical reactions as the low-component systems (chiefly for petrological purposes).[4] Admittedly, the language of such a nearly abstract mathematical treatment is unusual; therefore, many attempts have been made to simplify the problems. This is possible if one restricts oneself to certain well-chosen selection principles, e.g., by comprehending isomorphous groups of the ingredients in silicate materials[5] (a method chiefly used in mineralogical and petrological classification systems),* or by discussing only low concentrations in one or the other "added" components. In cement chemistry this latter principle is particularly useful and was, for example, adopted by W. R. Eubank[6] in his studies on the role of alkalies in cement mixes, and by Swayze[7] in discussing the effects of magnesia in normal cement mixes of the system CaO-Al_2O_3-Fe_2O_3-SiO_2. For this latter system, F. M. Lea and R. Parker[8] have given a three-dimensional space diagram to which Swayze had only to add certain polydimensional "sections" for constant additions, e.g., of 5 percent MgO, to give a most impressive projection of the Rankin-Lea-Parker diagram, modified by the presence of MgO. Eubank's studies on the effects of the alkalies illustrate another simplifying method by restricting also the additions in R_2O (as NC_8A_3) to different values in successive planes. He designed a tetrahedral "partial space" diagram for this

*The well-known chemical classification systems of P. Niggli and A. N. Zavaritskii are based on "chemical parameters" based on such selection principles. They are especially helpful in the difficult investigation of magmatic differentiation cycles.

restricted concentration field which makes sufficiently clear the polynary crystallization paths in the concentration area which is important for the production of industrial portland cements.

Not many of us will be in the position to use polynary diagrams in daily routine. For most practical problems it is, fortunately, not necessary to do so. There are ways of simplifying the discussions by the restriction to other partial problems which are only a portion of the general solution indicated above, but may therefore be given in an easier readable form. In this respect we are most grateful to Mr. Spindel[9] for his "fourmatter-parallelogram" which has the advantage of being feasible for routine work in the plant and for the civil engineer.* It is an important help for this purpose that simple rectangular coordinates may be used in the place of homogeneous coordinate systems with other than right angles. The fundamental idea is to derive from adequately projected sections of the polynary diagram (not to be discussed in detail here) the tie-lines (in the polynary theory usually called the conodes). The quantitative ratios of distances of projection positions give the great advantage of making possible a direct calculation of raw material batches, and of the resulting products of given silicates, aluminates, etc., by simple length measurements of lines and their ratios. For use in the plant, G. Fenaroli[10] and Q. Sestini[11] worked out corresponding simplified projections which are in principle nothing but the nomographic solution of calculations to derive the so-called empirical moduli of composition like those proposed by H. Kuehl.[12] The ingenious systems of equations given by R. Dahl[13] for the calculation of accurate cement compositions and products are, on the other hand, nothing but the corresponding analytical method for the solution of the same problems which in "polynary space" nomograms are solved graphically.

Coming back to the rich material given in the investigations of Jones and D'Ans and Eick on the system CaO-Al_2O_3-$CaSO_4$-H_2O we see immediately that this quaternary system has a character somewhat different from that of the dry melt equilibria systems mentioned above. The method of projection used by Jones and D'Ans and Eick is that of a reciprocal salt solution and crystallization equilibrium as mentioned on pp. 680 ff. Evidently, we are concerned with an apparently different class of polynary systems in which one component, namely the water, is constantly present. If one apex of an n-dimensional projection is pure H_2O, then it is evident that unsaturated solutions will immediately be represented in the n-dimensional variety of concentrations in the "n-dimensional space" near this apex. It depends on the choice of the investigator to arrange the other components of the systems in question in the simplest possible projection. The proficiency of J. H. van't Hoff in his classical investigations on the crystallization equilibria of oceanic salt solutions[14] was chiefly based on the excellent choice he made for illustrating in "partial" diagrams the highly complex equilibria in a system which may be indicated by the ionic symbols $(Na^+ - K^+ - Mg^{2+} - Ca^{2+}) - (Cl^- - SO_4^{2-}$

*Spindel considered also the introduction of $CaSO_4$ (as gypsum) into the polygon projection as a sixth component to fully describe all the phases new-formed in set and hardened cement. The author, however, is not certain whether this extension of the fourmatter-parallelogram was performed and discussed by Mr. Spindel in later publications.

$-BO_3^{3-}$)—H_2O, if we omit the less important components (Br^-, Fe^{2+}, etc.). One of the most adequate projections of this kind was that first used by van't Hoff's co-worker R. Löwenherz[15] for the partial system (K_2^{2+} $-Mg^{2+}$)—(Cl_2^{2-} $-SO_4^{2-}$)—H_2O, using a quadratic pyramid (half an octahedron) as the "representing space," and projecting the quantitative compositions of given solutions and phases in the direction of the space diagonal on the quadratic basis. A slight variation of this typically reciprocal salt equilibrium system is that later given by van't Hoff and W. Meyerhoffer (Reference 14, pp. 77 ff), with the additional condition of constant saturation of the salt NaCl, and of electroneutrality of the solutions (excluding an acid or base content). Especially E. Jänecke[16] and H. E. Boeke[17] worked out the modern projection methods for the oceanic salt equilibria, and J. D'Ans[18] in his fundamental modern discussion of the salt systems of practical importance in the potassium industry adopted another simplified projection in a triangular pyramid which was also used in preference in our present article.*

The readability of D'Ans' and Eick's original diagram suffers somewhat, although it is the quantitatively correct way to handle every projective-nomographic solution of the actual problems, by the great distortion of the dimensions caused by the enormously different solubilities of lime hydrate, alumina hydrate, and calcium sulfate. This is why the author used a logarithmic scale for a redesigning of the results of D'Ans and Eick. The derivation of quantitative relations from the logarithmic diagram cannot have the rank of accuracy and simplicity as the corresponding nomographic operation of the original diagram. While in the latter the tie-lines between fixed points, and the ratios of concentrations in the components, or of special compounds of the system may be used directly, one must not forget that by the logarithmic scales of Fig. 9 and 10 the tie-lines are distorted to hyperbolae, and the ratios of lengths on these curves must be converted to numbers. The practitioner will therefore give preference for calculation purposes to D'Ans' and Eick's original diagram, the teacher to the logarithmic projection.

Undoubtedly the plant chemist and the civil engineer who will study the extensive material given by Jones and D'Ans and Eick will benefit from getting acquainted with the general possibilities of crystallization of complex salts from apparently simple mixes in the presence of water. If further it is never forgotten that equilibrium diagrams are idealizations of what in nature and in a technological system may be much changed by nonequilibria, the practitioner will understand how much still has to be done to bring the ideal numerical data into agreement with the requirements of his special purposes. A first indication of what nonequilibria may mean in our case is well illustrated by the Fig. 9 and 10 showing the great differences in the behavior of alumina hydrates in the course of "aging" reactions. Such time-related reactions are what the civil engineer will be particularly interested with.

*It will be profitable for the reader to study carefully the introductory derivations in D'Ans' book[18], pp. 25-29, on the reciprocal equilibria, and the interrelations between the modified Löwenherz projection method, and the central projections used by Jänecke and Boeke. The first has the particular advantage of a good spatial impression.

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Discussion of a paper by Sven Thaulow:

Tensile Splitting Test and High Strength Concrete Test Cylinders*

By P. W. ABELES, IQBAL ALI, E. GRUENWALD, HUBERT RÜSCH and GUNNAR VIGERUST, and AUTHOR

By P. W. ABELES†

The writer will limit his contribution to the author's suggestion that cylindrical specimens should be accepted generally for compression and tension tests. Unfortunately, in Europe most of the specialists have not yet recognized the superiority of cylinder and prism tests as compared with cube tests. Comparative tests have clearly indicated that there is no direct relationship between these strength properties (if there were, there would obviously be no necessity to determine in addition the cylinder or prism strength). The ratio between the cylinder or prism strength and the cube strength may normally vary between 0.6 and 0.9. There is no hard and fast rule that this ratio may depend on the strength or on the water-cement ratio, or any one factor; it may happen that with high strength concrete of one kind the ratio is 0.9 and with that of another type it is 0.6. Cylinders might be preferable to prisms since it is possible to use them also for tensile tests as described by the author.

As this question mainly concerns Europe, it would be of importance to convince concrete specialists in this part of the world to introduce cylinders instead of cubes. It would be of benefit if the appropriate institutions of European countries could introduce jointly a uniform kind of concrete testing, using the same type and size of specimen, i.e., cylinders of 6-in. diameter and 12-in. depth, to which size all non-European countries could also agree. It should be pointed out that at present even with regard to cubes used in Europe, the size is not uniform, e.g., in Great Britain 6-in. cubes are used mainly, while the standard size of cubes employed in the remaining countries is approximately 8 in.

Nevertheless, it is of great importance that the variable relationship between cube and cylinder strengths be appreciated in the United States, since erroneous conclusions are sometimes drawn from an assumed constant conversion factor.

*ACI Journal, Jan. 1957, *Proc.* V. 53, p. 699. Disc. 53-38 is a part of copyrighted Journal of the American Concrete Institute, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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By IQBAL ALI*

It was mentioned in the concluding remarks that the use of cylindrical specimens might also be considered for testing of cement. Some tests† were conducted by the writer at the Engineering Research Department, Hyderabad, India, in this connection. These indicated that the cylinder splitting test, made on cement-sand mortar, had a degree of reproducibility considerably higher than that obtainable either with beam tests or the standard tension test using figure-of-eight briquets.

The great convenience of being able to use the same type of specimen and the same equipment for compression and tension tests, together with the higher uniformity of the results, constitute a strong argument for adoption of the cylinder splitting test as standard procedure for testing cement.

By E. GRUENWALD‡

Our research laboratory carried out an exploratory investigation of the indirect tensile test of concrete to determine its reproducibility.

Type I and Type III cements were used in five mixes containing 3.6, 4.5, 6, 7.5, and 9 bags of cement per cu yd of concrete. Ten 6 x 12-in. cylinders

TABLE A—SPLIT TENSILE TESTS AND COMPRESSION TESTS OF 6 X 12-IN. CYLINDERS

Mix—bags per cu yd	3.6		4.5		6		7.5		9	
Type of test	T.S.	C.S.	T.S.	C.S.	T.S.	C.S.	T.S.	C.S.	T.S.	C.S.
Concrete with Type I cement										
Age at test, days	7	7	7	7	7	7	7	7	7	7
	222		246		369		422		377	
	237		252		389		395		382	
	211		321	2940	316	4210	316	4990	276	6110
	230	2050	273	3030	290	4240	431	5120	308	5900
	207	1820	338	3030	353	4260	361	5140	331	5890
	196	2050	244	2940	305	4200	446	5260	353	6040
Average	217	1973	279	2985	337	4227	395	5127	338	5985
Standard deviation, psi	15	111	37	45	36	18	45	90	37	93
Coefficient of variation	7	6	13	2	11	0.4	11	1.8	11	1.6
Concrete with Type III cement										
Age at test, days	6	6	6	6	6	6	6	6	6	6
	290		316		448		482			
	195		352		326		437		411	
	278		304		353		431		368	
	283		305		314		423		422	
	225	2850	358	4230	428	4960	391	6160	214*	6640
	267	2890	291	4120	327	5130	532	5810	338	6640
	247	2820	337	3870	372	5080	443	6250	405	6580
Average	255	2853	323	4073	367	5057	448	6073	389	6620
Standard deviation, psi	32	37	25	153	48	66	44	192	31	28
Coefficient of variation	13	1.3	8	3.8	13	1.3	10	3.2	8	0.4

*Not included in average.

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†Ali, I., Lingam, A. B., and Ramchander, K., "A Method of Determining Tensile Strength of Cement Mortar," *The Indian Concrete Journal* (Bombay), V. 30, No. 4, Apr. 1956, pp. 106-108.

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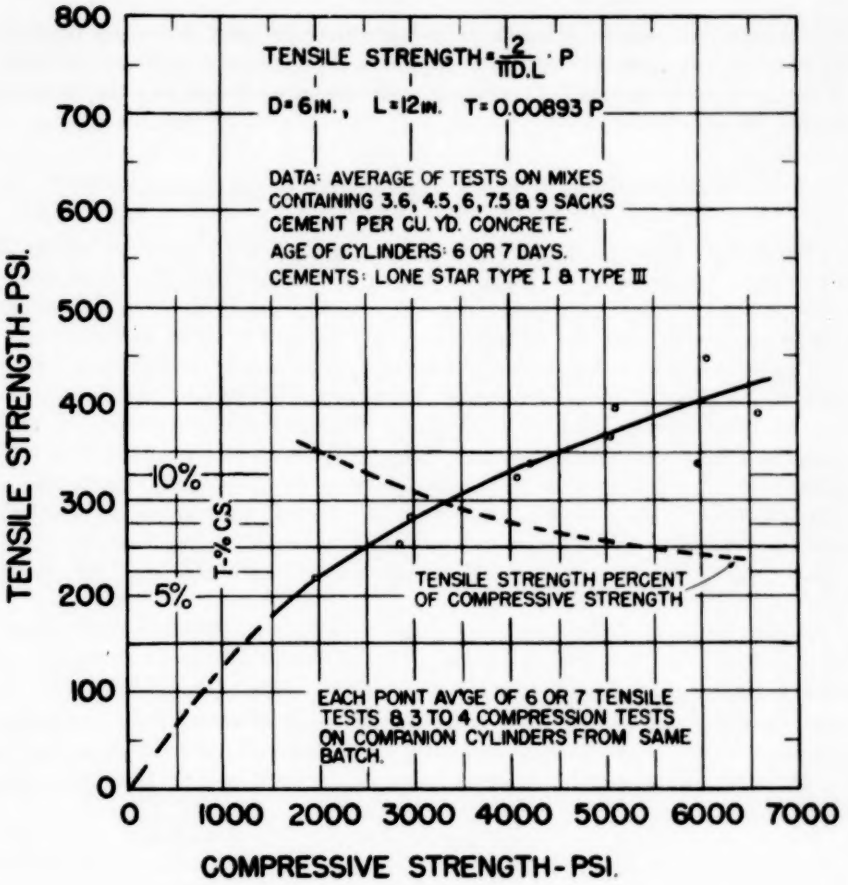

Fig. A—Split tensile tests of 6 x 12-in. cylinders

were molded from each batch. Cylinders were moist cured until test at 6 days for the Type III cement and 7 days for the Type I cement. Six to seven cylinders from each batch were broken for tensile strength by applying compression on opposite generatrices. Rawhide strips ½ in. wide and ⅛ in. thick were used top and bottom to distribute load. Three to four cylinders from the same batches were broken in compression.

As these tests were made from the same batches, the results indicate what variations may be expected within a batch due to the test proper. Standard deviation (Table A), excepting the 3.6-bag mix with Type I (σ = 15 psi), ranged from 25 to 48 psi without showing any relation to strength level. The coefficient of variation ranged from 7 to 13. On the other hand, compression results were extremely uniform.

The results showed a fairly consistent relation between split tensile strength and compressive strength as indicated in Fig. A.

The ratio of tensile strength to compressive strength decreased from 11 percent for the 2000-psi concrete to about 7 percent for 6000-psi concrete. The curve shows this ratio lies between the extremes found by other investigators shown on Fig. 3.

By HUBERT RÜSCH* and GUNNAR VIGERUST†

The writers have not had any extensive opportunity to apply the proposed method for determining the tensile strength of concrete by splitting cylinders, and are therefore not in a position to comment in detail on all the questions which are posed when the suitability of this method is to be evaluated.

The problems were encountered indirectly, however, while the writers were conducting a series of test studies of diagonal tension in prestressed concrete at the Technical University of Munich. These studies have not yet been published, and the writers are only presenting those test results which it is presumed will be of interest in connection with the discussion. As failure of concrete beams, both prestressed and nonprestressed, in diagonal tension is a function of the tensile strength of the concrete, much emphasis was given in these tests to the determination of tensile strength of concrete as accurately as possible.

Concretes used for the tests were of the strengths which the German standards prescribe for prestressed concrete, namely those giving 4300, 6400, and 8500 psi in compression after 28 days, using 8-in. cube specimens. It will be seen that the test results cited (Fig. B) are grouped about these strengths. The test schedule permitted determining the compressive and tensile strengths of 11 mixes, all strengths in turn being determined as the mean of three parallel tests.

For a comparative study the tensile strength was determined (1) as the tensile splitting strength of 6 x 12-in. cylinders, and (2) as the tensile flexure strength of 4 x 6 x 28-in. beams. Specimens for the tensile splitting tests were prepared as proposed by Thaulow[1] and described in his JOURNAL article. Two strips of 15-mm pliable fiberboard were inserted between the specimen and the loading heads to distribute the load. Flexure strength was determined according to German standard specifications, with a 24-in. span and one concentrated load at midspan. Compressive strengths were determined using both 8-in. cubes and 6 x 12-in. cylinders, the latter prepared according to Thaulow's proposal. All test specimens had similar curing conditions, the first week under moist burlap and thereafter three weeks in laboratory air until tested.

The small number of test specimens involved permit only a trend to be noted from the results. More extensive tests are necessary, and specimens should be cured in a humidity chamber under strict control. It should also be pointed out that only a limited portion of the range of strengths generally

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used for reinforced concrete was covered by the study. On the other hand, the strengths studied lie in a range where little experimental study has been made of the tensile strength, i.e., corresponding to compressive strengths of cubes between 4000 and 9000 psi. The writers therefore believe that their test results can be of some interest, all the more since they permit certain conclusions regarding the increasing elastic behavior of high strength concretes.

All test results are briefly presented in Fig. B. It will be noted that within the range of strengths studied there is a somewhat constant ratio, a mean of 92 percent, between the cylinder and cube strengths. In earlier tests Rüsch[2] also found an approximate, constant ratio between cylinder and cube strength, independent of the concrete strength. He studied a large number of compressive strengths ranging from 1400 to 10,000 psi (cube strength), and found the ratio equal to 88 percent. It is, however, generally assumed by those who have studied this problem that the ratio of cylinder to cube strength is quite dependent on the strength of the concrete.

Fig. B also shows the relationship between tensile flexure strength and cube strength, and between tensile splitting strength and cube strength, the tensile strengths given as a percentage of the cube strength. The ratio of tensile flexure strength and tensile splitting strength to compressive strength decreases with increasing compressive strength. This is also pointed out in Thaulow's article, and is otherwise generally known. But the writers' results do not confirm Thaulow's assumption that the tensile splitting strength is a constant proportional part, for example 60 percent, of the tensile flexure

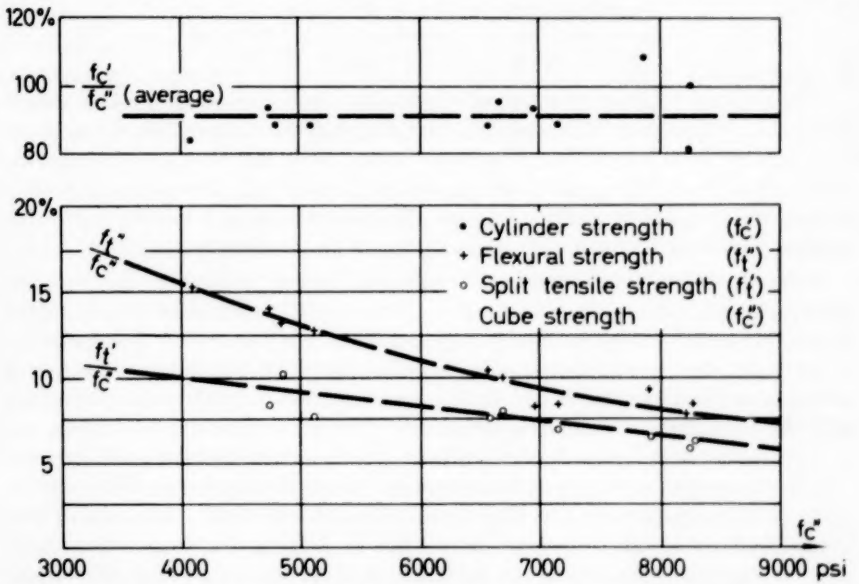

Fig. B—Ratio of cylinder, flexural tensile, and split tensile strengths, respectively, to cube strengths as a function of cube strengths (Munich tests)

strength. This can be confirmed for a cube strength of about 4300 psi, but the ratio approaches unity for very high strength concretes.

The writers will now pose three questions which must be answered to properly evaluate the method for tensile strength determination:

(a) Is it more practical to determine the tensile strength by a tensile splitting test than by a flexure test?

(b) Does the tensile splitting test show a smaller dispersion of test strengths than the flexure test?

(c) Does the tensile splitting strength provide a more correct expression for the *true* tensile strength of the concrete than the tensile flexure strength?

Is it more practical to determine the tensile strength by a tensile splitting test than by a flexure test?

Thaulow has pointed out the practical advantages of the tensile splitting test, for example, that specimens of the same shape can be used for both the compression and the tension test. There is probably a consensus that for this reason the tensile splitting test should be preferred.

Does the tensile splitting test show a smaller dispersion of test strength than the flexure test?

At this writing it is not possible to give an unqualified affirmative answer to this question. As mean values for all their test mixes the writers found the following coefficients of variation in strengths:

compressive strengths	—cylinders: 3.5 percent
compressive strengths	—cubes: 2.5 percent
tensile strengths	—tensile splitting: 6.0 percent
tensile strengths	—tensile flexure: 4.6 percent

The dispersion of compressive strengths was thus about half that of tensile strengths. In general it is assumed that the dispersion in tensile strengths is considerably higher than found by these tests. It is also seen that the dispersion in tensile splitting strengths is *not* lower than the dispersion in tensile flexure strengths, but slightly higher. Thaulow[1] found about the same dispersion in tensile splitting strengths as in the tensile flexure strengths.

However, the writers would point out that actual control at the building site can change this picture radically. It is well known that the apparent flexure strength will be markedly affected if the surface of the test specimen is permitted to dry out and shrink, but that the effect on the tensile splitting strength is much smaller under similar circumstances. In the tensile splitting test the maximum tensile stresses occur in the inner fibers of the specimen, and in the outer fibers in the flexure test. In the latter case a dry surface will reduce the apparent strength considerably. Such drying out is not likely to occur in laboratory tests, but may readily occur in the field. The same effect is obtained by minor, local defects in the concreting, for example, a large piece of aggregate at or near the surface. Such a large stone near the tensile edge of a test beam will cause a considerable reduction in the tested flexure strength, but would be of less importance in the tensile splitting test. These

minor defects are apparently easy to avoid in carefully performed laboratory tests, but may occur often at the building site.

It is for this reason that the writers would caution about any conclusion on the comparative dispersion of strengths in flexure and tensile splitting tests based only on laboratory tests. It is probable that in practice the dispersion in strengths will be greater in the flexure test than in the tensile splitting test.

Does the tensile splitting strength provide a more correct expression for the *true* tensile strength of the concrete than the tensile flexure strength?

It is generally agreed that the tensile flexure strength is strictly a computed magnitude which does not express the true tensile strength of the concrete. The tensile strength of the concrete is therefore frequently set as a certain percentage of the strength found from a flexure test, for example 60 percent. For several reasons such an assumption is rather dubious. For one thing, the tensile flexure strength is not constant with variations in the cross section of the test beam, nor, for another, is it independent of the loaded span. The latter may be explained theoretically taking into account the theory of probability (Craemer[3]). When the distance between the load points is great, the mathematical probability that a weak point with reduced tensile strength will fall within an area with maximum tensile stress is much greater than with a concentrated load at midspan. The tensile flexure strength in the first case would therefore appear lower than in the latter.

Both of these factors, however, have little significance for the tensile splitting test. Thaulow[1] has shown experimentally that the tensile splitting strength is virtually independent of the dimensions of the test specimen.

What is just as important, however, although probably little known, is that the deviation of the tensile flexure strength from the true tensile strength varies with the strength of the concrete. This can be explained by studying the plastic and elastic properties of the concrete at different compressive strengths.

In computing the tensile flexure strength it is assumed that Hooke's law is applicable to failure, with a triangular distribution of stress on the cross section. This assumption is satisfied to various degrees by concretes of different strengths. High strength concretes have elastic properties almost to failure, the assumption of linear stress variation being approximately satisfied. On the other hand, low strength concretes have marked plastic properties, and there is an approach to a uniform stress distribution over the section, instead of the triangular distribution. For this reason the general assumption that the true tensile strength of the concrete is about 60 percent of the tensile flexure strength independent of the strength of the concrete is also in error.

This will be seen in Fig. B, where the ratio of tensile splitting strength to tensile strength is about 70 percent for a cube strength of 4300 psi, approaching unity with increasing cube strength. For the time being we shall tacitly assume that the tensile splitting strength is actually equal to the true tensile strength.

In an earlier study Rüsch[2] attempted to draw certain conclusions about the "ideal-plastic" and "ideal-elastic" limiting conditions of the concrete. From

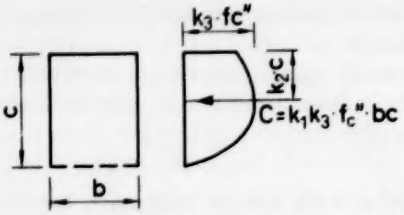

Fig. C—Stress block of the compression zone in ultimate bending

Notations:

f_c' = cylinder strength
f_c'' = cube strength
f_t' = split tensile strength
f_t'' = flexural tensile strength
$(k_1k_3)'$ = factor (k_1k_2) for constant values of $k_2 = 0.333$

a study of the ultimate strength of the compression zone in bending, Rüsch concluded as follows, with notations as given in Fig. C.

An *ideal plastic* material will be characterized by a uniform distribution of stress in the compression zone at failure; the stress will be equal to the prism (cylinder) strength. The resultant compressive force acts therefore at the center of the compression zone ($k_2 = 0.5$). The ratio of the equivalent ultimate stress in the compression zone to the cube strength will be equal to the ratio of prism strength to cube strength, experimentally found in his tests as $k_1k_3 = 0.88$.

In an *ideal elastic* material the stress varies linearly with its distance from the neutral axis and the resultant compressive force acts at the upper third-point of the compression zone ($k_2 = 0.333$). Unfortunately it is not possible to be equally certain about the ratio of the equivalent ultimate stress to cube strength (k_1k_3) for this limiting case. It does not follow that the outer fiber stress in this case would coincide with either the cube strength or the prism (cylinder) strength.

In Fig. D the coefficients k_2 and k_1k_3 are shown as functions of the cube strength, based on Rüsch's experiments. To facilitate the extrapolation of the curves toward the desired limiting values the cube strength is shown in a root scale. It is apparent that the curves satisfy the ideal plastic conditions if the compressive strength of the concrete approaches zero, where k_2 approaches 0.5 and k_1k_3 approaches 0.88. To obtain a limiting value for the ideal-elastic concrete too, experimental values of k_1k_3 for concretes of different strengths have been shown on the diagram, valid for a force acting with $k_2 = 0.333$ at the upper third-point. The ideal-elastic concrete will have been reached when the last curve [designated $(k_1k_3)'$] coincides with the curve for k_1k_3, found for the ultimate strength of the compression zone in bending. It will be seen that a cube strength of 15,000 psi would be necessary to arrive at an ideal elastic behavior.

The formula used to compute the tensile splitting stress is developed mathematically on the assumption that the concrete obeys Hooke's law. The same assumption is also the basis for computing the tensile flexure strength, but it is only fully valid for the *ideal-elastic* concrete. For such a concrete both the tensile splitting test and the flexure test will actually yield the same value for the tensile strength, equal to the true tensile strength. In Fig. D the ratio of tensile splitting strength to flexure tensile strength ($f_t' : f_t''$) has been drawn

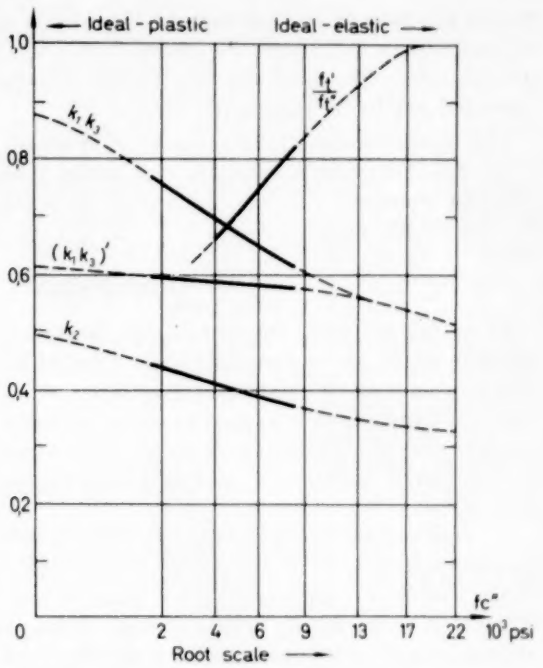

Fig. D—Characteristic properties of the ideal-elastic and the ideal-plastic concrete as influenced by the compressive strength

——— Test values
– – – – – Extrapolated values

in as a function of the cube strength. By extrapolating the curve to $f_t' : f_t'' = 1.0$, it should be possible to find the cube strength necessary to reach an ideal-elastic concrete. This occurs for a cube strength of about 17,000 psi, a value agreeing well with that previously found for flexural compression.

Even if these suppositions are based on uncertain assumptions, the results may well be taken as proof that very high strength concretes have almost completely elastic properties.

In Fig. D the curve for $f_t' : f_t''$ is obviously determined for such a small range that the extrapolation toward a cube strength equal to zero would be dubious, the more as it would be impossible to obtain the limiting value for $f_t' : f_t''$ in this case.

Hereto we have assumed that the tensile splitting strength of the concrete is actually equal to the true tensile strength. But is this so? The stress distribution in the tensile splitting test computed on the assumption of an ideal elastic material is virtually uniform. Only a slight adaption is therefore necessary to conform to the fully uniform distribution occurring with complete plasticity, and this would not appreciably affect the result of the tensile splitting test. Thus, for higher strength concretes the tensile splitting strength is probably quite close to the true tensile strength of the concrete. An indirect proof of this was furnished by the writers from their shear tests with prestressed concrete beams. With good approximation they found it possible to compute the bending moment for the first flexural cracks by equating the

tensile strength of the concrete to the tensile splitting strength. For lower strength concretes it must be assumed that the tensile splitting strength will deviate somewhat from the true tensile strength, although the deviation is supposed not to be particularly large.

The tensile flexure strength, however, as shown above, will deviate markedly from the true tensile strength, this being more pronounced for the lower strength concretes. In the opinion of the writers this is a strong argument for the tensile splitting test.

CONCLUSIONS

In actual building site control the determination of the tensile splitting strength would be far less likely to be affected by drying out of specimens and minor concreting defects than the flexure test. It must therefore be assumed that the dispersion of strengths would be greater for the tensile flexure strength than for the tensile splitting strength. This would not be noticed in laboratory results, careful handling of the test specimens tending to eliminate such chance defects.

It may not be assumed that the tensile splitting strength is a constant, proportional part of the tensile flexure strength independent of the strength of the concrete. Test results show that the tensile splitting strength is about 70 percent of the tensile flexure strength for a cube strength of 4300 psi probably approaching 100 percent for a hypothetical cube strength of 17,000 psi. It may be assumed that the tensile splitting strength lies fairly close to the true tensile strength of the concretes, particularly for the higher strength concretes.

These observations are supported by an analysis of the limiting conditions at the ideal-elastic and ideal-plastic concrete. This in turn provides an indirect proof that concrete with very high compressive strength is almost completely elastic.

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AUTHOR'S CLOSURE

The comments that this article has occasioned would indicate that the subject is of universal interest.

It is interesting to note that in India, according to Iqbal Ali, tensile splitting strength determinations are used in cement tests with satisfactory results. An article has also been received from Australia[1] showing that there is an interest in this testing method.

Relationship between compressive strength, tensile strength, and tensile splitting strength is treated in the discussions by E. Gruenwald, and H. Rüsch and G. Vigerust. The tests made in Munich, which the latter mention, are of particular interest in that they embrace studies using high strength concretes. In an era of prestressed concrete this is quite timely. It is a common but erroneous assumption that the tensile splitting strength is a constant proportional part of the tensile flexure strength. The explanation that Rüsch and Vigerust give for this based on the elastic properties of the concrete makes it clear that the actual tensile strength of the concrete is better indicated by the tensile splitting strength than by the tensile flexure strength.

The author can only second Mr. Abeles' proposal that the cylinder be introduced as an international standard, but that in a transition period both the cylinder and the cube should be accepted, as is practiced today in the Scandinavian countries (Denmark, Finland, Norway, and Sweden).

In conclusion the author would like to quote what one of the participants in the discussion has said upon another occasion:[2]

> "By comparing the advantages and disadvantages of both types of specimens it is quite apparent that the cylinder test should be selected for the much-desired international comparisons. It is true that shifting to cylinder testing has been rejected in Germany several times after thorough argument. But it is not to be overlooked that two new factors of vital significance have since appeared, to wit, the introduction of Thaulow's method for preparing the cylinders and the introduction of the splitting test for tensile strength determinations."

The author would also quote C. Zelger:[3]

> "The enumerated advantages of cylinder testing, as far as we can judge today, have few drawbacks. It is therefore to be expected that also in Europe the cube for compression testing and the beam for flexure testing will sooner or later be replaced by the cylinder."

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Discussion of a paper by James S. Minges and Donald S. Wild:

Six Stories of Prestressed Slabs Erected by Lift-Slab Method*

By IRWIN J. SPEYER†

The writer congratulates the authors on their comprehensive discussion of the design of the Litchfield Hospital. As a member of the engineering team, we found this structure a particular challenge both in the design stage, and later, in its construction.

We believe it will be of general interest to enlarge upon a number of points touched upon in the article, and introduce a discussion of flexural safety factor.

LOAD DISTRIBUTION

As the authors indicate, a triangular load distribution was assumed prior to carrying out the bent analysis. Fig. A illustrates how the uniform loading was apportioned between the longitudinal and transverse directions.

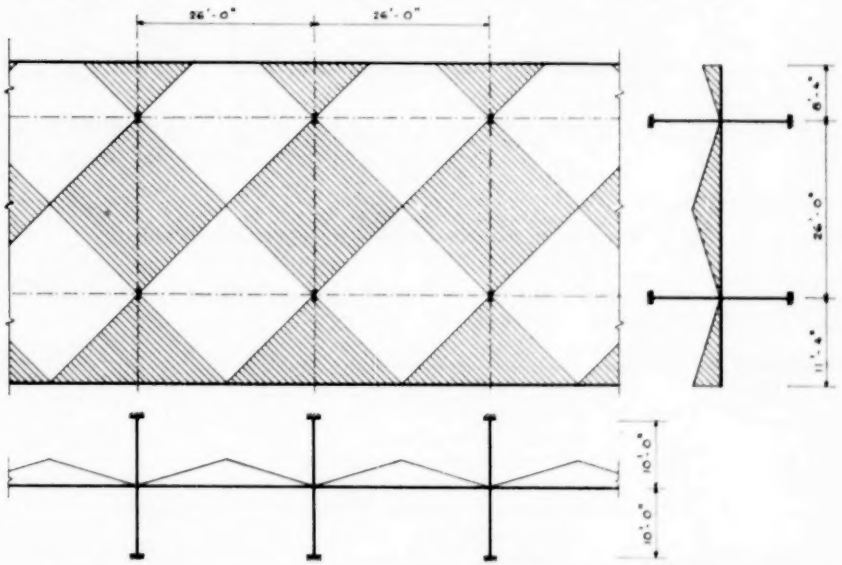

Fig. A—Loading apportioned between longitudinal and transverse directions

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†Freyssinet Co., Inc., New York, N. Y.

The total positive and negative moments were then computed by the Hardy Cross method, and distributed to the column strip and middle strip as indicated by the authors.

FLEXURAL SAFETY FACTOR

At ultimate load, a prestressed concrete structure will act as a reinforced concrete beam, with the prestressing tendons carrying all tensions. In the case of a continuous beam, the section of maximum moment will yield, and there will be a distribution of moments. The structure will collapse only when a "mechanism" develops.

In the longitudinal direction, there are 20 post-tensioning cables (each cable composed of ten 0.196-in. diameter wires), each with a steel area of 0.302 sq in. In addition, centered over each column there are six #6 and two #8 bars. The location of the cables and bars is as shown in Fig. B.

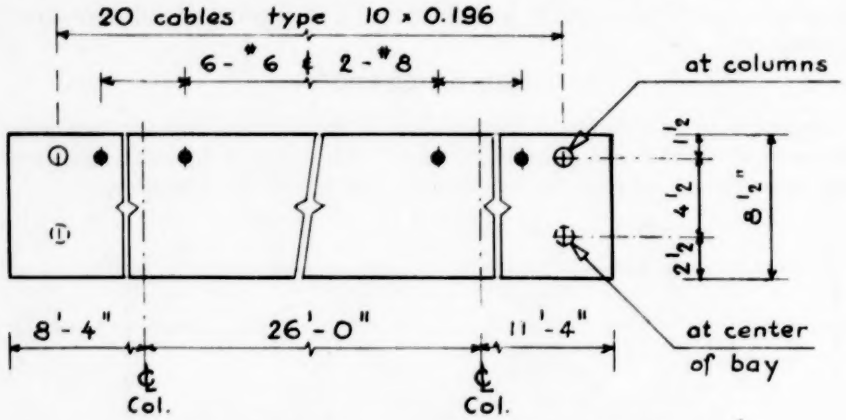

Fig. B—Location of bars and cables in longitudinal direction

The ultimate moment may be computed by the method recommended in ACI 318-56, as follows:

At center line columns

Cables:
$A_s = 20 \times 0.302 = 6.04$ sq in.
$bd = 43 \times 12 \times 7 = 3620$ sq in.
$p = 6.04/3620 = 0.00167$
$f_{su} = 0.8 \times 230{,}000 = 184{,}000$ psi
$q = 0.00167 \times 184/4.5 = 0.068$
$M_u = 3620 \times 7/12 \times 4.5 \times 0.068\,(1 - 0.59 \times 0.068)$
$= 616$ ft-kips

Reinforcing bars:
$A_s = 8.44$ sq in.
$bd = 3620$ sq in.
$p = 8.44/3620 = 0.00233$
$q = 0.00233 \times 40/4.5 = 0.0207$
$M_u = 3620 \times 7/12 \times 4.5 \times 0.0207\,(1 - 0.59 \times 0.0207)$
$= 194$ ft-kips

At center line span

Cables:

A_s = 6.04 sq in.

bd = 6 × 43 × 12 = 3110 sq in.

p = 6.04/3110 = 0.00194

q = 0.00194 × 184/4.5 = 0.0794

M_u = 3110 × 6/12 × 4.5 × 0.0794 (1 − 0.59 × 0.0794)
= 526 ft-kips

Total M_u = (616 + 194 + 526) 0.9* = 1200 ft-kips

Live load design moment = 110 ft-kips
Dead load design moment = 372 ft-kips
Flexural safety factors = 2.50 (D.L. + L.L.)
= D.L. + 7.50 L.L.
} in longitudinal direction

DEAD END ANCHORAGE

Along one longitudinal edge the architects provided an eyeshade 3 ft wide, as can be seen in Fig. 2. It was considered inadvisable and uneconomical to block out at each transverse cable to reach the anchorage. Therefore, all non-jacking ends were anchored by bonding the bare wire to the concrete, as indicated in Fig. C.

At the time of jacking, the maximum stress at the jack was a maximum of 0.80 f_s', or 184,000 psi. The stress at the dead end, using the friction factors enumerated in the text, is approximately 160,000 psi. The total length of embedded wire is 10 × 45 in. = 450 in. Therefore the average bond stress is:

$$u = \frac{160{,}000 \times 0.0302}{450 \times \pi \times 0.196} = 174 \text{ psi}$$

This value is only slightly above the working stress in bond as allowed by ACI 318-56 which, for 4500-psi concrete, is 158 psi.

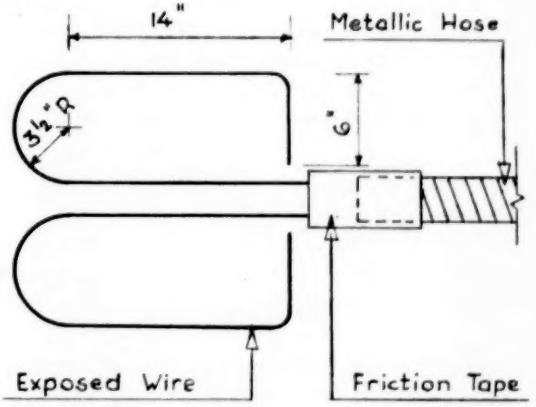

Fig. C—Dead end anchorage, plan view

*10 percent reduction to account for the possible incomplete redistribution of moments.

Discussion of a paper by F. N. Hveem and Bailey Tremper:

Some Factors Influencing Shrinkage of Concrete Pavements*

By E. L. HOWARD, LEWIS H. TUTHILL, T. F. WILLIS and L. T. MURRAY, and AUTHORS

By E. L. HOWARD†

This discussion concerns itself with only the "sand equivalent" portion of the paper. In the matter of the importance of "clean sand" to highway concrete work there is no disagreement. This discussion presents a sand producer's approach to the use of the sand equivalent test procedures in quality control.

Three men, making sand equivalent tests on concrete sands at the plant, tested a portion of the same sample. (A typical comparative result is shown in Table A.) The height of the "sand line" reading in each test was more nearly the same than the "clay line" readings. In the example, Table A, this difference in clay line placed one test outside the specification limits. The inability of plant technicians to duplicate test results made the sand equivalent test useless for field control. During the period these three men worked together, three variables were noted. First, the volume of the sample tested differs unless the measuring can is filled and struck off carefully each time. Second, the clay line level is affected by the shaking procedure. Third, the sand line is reported at the point a weighted foot sets firmly on the sand. A slight twisting motion, common with some operators, in setting this foot will lower the level of the sand reading.

TABLE A—COMPARATIVE RESULTS OF SAND EQUIVALENT TESTS FROM IDENTICAL SAMPLES

Technician	WEM	ME	JDG
Clay line reading	6.1	5.2	5.5
Sand line reading	4.1	4.2	4.1
Sand equivalent	68	81	75

The technicians doing this study adjusted their working habits in close conformation to the standards. Each measured the samples with care and timed the shaking procedure to 90 strokes in 30 sec. Each man set the weighted foot without twisting. Close correlation of test results immediately followed these improved testing conditions with a typical group of tests made by these three men yielding sand equivalents of 78, 75, and 76.

The field men had worked together during this study and could observe each others' technique. This practice gave an obvious advantage to the con-

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formity of test procedure and results. With this fact in mind, a man was assigned to each of two field laboratories, and each tested a different sand. Mr. Elsnab tested a sand yielding results on the borderline of specifications. In the other field laboratory, Mr. Gordon tested sand that was inside the specification limits. Both sands were produced from the same general area. Mr. Gordon made three tests and Mr. Elsnab made two tests of each sample. The uniformity of the clay line readings and the sand levels within each of these two and three test groups are considered a fair measure of the efficiency of the sand equivalent test. Working under typical field conditions, these men put the sand equivalent test to trial as a measure of quality control.

TABLE B—SAND EQUIVALENT TEST DATA COMPARED—VARIATION OF WITHIN TEST GROUPS

Technician	JDG	ME
Clay line reading		
Deviation	0.2	0.3
Variation, percent	4.1	5.8
Average	4.9	5.2
Sand line reading		
Deviation	0.1	0.1
Variation, percent	2.6	2.6
Average	3.9	3.9
Sand equivalent		
Deviation	2.7	2.5
Variation, percent	2.6	2.6
Average	80	75

Because each man used the utmost care and made the test in the standard manner, their efficiency was equally good (Table B). Possibly a 3-5 percent variation is the best that can be expected with the sand equivalent test as now standardized. Generally, the variation between operators is much greater, as Table A indicated. The greater variance is in the clay line reading. The differences in this reading are affected by the work of the operator and also by the type and amount of claylike material in the sample. The sand tested by Mr. Elsnab contained greater amounts of this claylike material, making the duplication of clay line readings more difficult than with Mr. Gordon's "clean" sand.

To meet the standards as now set at a sand equivalent 75 average, with no sample less than 70, a sand must actually average a sand equivalent of 80. The variation of sand equivalents within the test groups is great enough to put a borderline sand outside of specifications as often as it is in. A sand averaging a sand equivalent of 75 might easily be tested at 70 or less, even by ordinarily careful technicians. Two such tests in a group of three would condemn the sand for specification work.

TABLE C—SUMMARY OF SAND EQUIVALENT VALUES FOR TWO SANDS

Sand	A	B
Number of samples	106	80
Average sand equivalent	80	75
Standard deviation	10	5
Variation, percent	12.5	6.7

The data in this study point out the possible uniformity attainable in sand production (Table C). Sand A conforms to the specifications, but the variation is greater than desired. It is an acceptable sand in this case because the average sand equivalent is greater than the specifications require. A small reduction in the average sand equivalent or the carelessness of a tech-

nician making the test will cause the material to be rejected. Sand B also meets the specification requirements, but only because the uniformity controls are extremely good. Sand B is on the borderline and can be used only so long as less than 5 percent variation in control is maintained.

The unexpected efficiency of the sand equivalent test as made by the men in this study suggests its possible use as a quality control tool at the production plant. The possible variations in testing procedures rob the test of all its value as a control tool, except in those cases where exacting test standards are maintained. Under ideal conditions 5 percent variation should be allowed in test results.

By LEWIS H. TUTHILL*

Shrinkage is so widely a problem in concrete performance and its effect on concrete pavements is sometimes so disturbing that the industry is indebted to the authors for the further light they have furnished on factors affecting it and how to detect them.

It is a commonly held concept that shrinkage correlates primarily with unit water content of concrete, and anything that affects the amount of water required for mixing directly somewhat proportionally affects the amount of drying shrinkage. Doubtless there are other contributing factors but probably they are minor. It may be one of these that is measured by the sand equivalent test or it may be that this test simply measures an aspect of aggregates which affects unit water requirement. It may be some of each.

Because it does not clearly appear for what reason results of the sand equivalent test correlate with drying shrinkage as well as the authors have shown, it would be interesting and helpful to see to what extent lower sand equivalent values correlate with higher water requirement for otherwise comparable mixes. If such a correlation does not exist, it would seem that dirt and clays have an unsuspected double effect to increase drying shrinkage.

By T. F. WILLIS and L. T. MURRAY†

There has been much debate among concrete technologists as to how well the so-called standard quality tests for concrete aggregate measure attributes of the aggregate which materially affect desirable properties of concrete. In the opinion of the writers the disagreement stems largely from the fact that many past experimenters studying the subject: (a) used experimental designs which give more or less biased samples of the population to which the experimental conclusions are to be applied; (b) failed to make provision for obtaining an estimate of the effect of extraneous, uncontrolled factors on the experimental results; and (c) used a vaguely defined, purely subjective basis for analysis and interpretation of the results of their tests. Each experimenter, and each

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of his critics, assumed a different scope for validity of the conclusions; each made an independent guess about the degree of influence of extraneous factors; and each adopted a different basis for interpretation of the results; so, confusion and disagreement were inevitable.

The authors of the paper under discussion have devised and studied a new test purporting to measure an aggregate attribute which influences the magnitude of drying shrinkage of concrete. Assuming that the range in drying shrinkage occurring in their experiment is indicative of concretes of varying quality (an, as yet, unestablished premise), it may be of interest to examine the experiment with respect to the points mentioned in our opening paragraph.

The design of the experiment is not described so it is not possible to tell whether the experimental results are an unbiased sample of the population to which the experimental conclusions are applied. With regard to points (b) and (c), however, the experimental work is on a firm footing. The use of statistical correlation analysis as the basis for interpretation of the test results provides an objective method for accomplishing this; objective, because the analysis is performed according to fully defined rules which were subject to no *a posteriori* influence either by the experimental data or by any prejudice of the analyst applying them. In addition, correlation analysis contains a built-in provision for estimating the degree to which extraneous factors are affecting test results. The paper, then, represents a distinct improvement over most prior papers along generally similar lines.

In what follows the discussors have attempted to clarify one of the authors' statistical statements, point out a computational error (or misprint), and develop some additional implications of the authors' data.

In describing the association between *drying shrinkage* and *sand equivalent*, the authors state: "The coefficient of correlation of this equation [Eq. (1)] is 0.66 which, for the number of tests involved, indicates a probability of better than 99 to 1 that the observed relationship is not due to chance." Considered literally, the statement is incorrect; and considered in its statistical connotation, misleading. Literally speaking the observed relationship *was* definitely the result of chance, the chance that the investigation would produce the specific set of measurements obtained rather than one of numerous other sets that might have been obtained. Statistically, the statement attempts to describe a probability derived from the statistical significance test, but fails to do so clearly. This probability pertains, not to the chance that the measured attributes are correlated (either they are or they are not), but to the chance that the set of experimental test result could have constituted a random sample from an uncorrelated population. What the significance test actually shows may be stated as follows: There is less than 1 chance in 100 that the authors' set of test results, considered as a random sample of some large population of such test results, could have emanated from a population in which the attributes "drying shrinkage" and "sand equivalent" were uncorrelated. It follows that the authors may then infer the attributes to be

correlated in the population they have sampled, with a risk of less than 1 percent that the inference is erroneous (or conversely, with 99 percent confidence that the inference is correct).

In connection with a discussion of the relation between drying shrinkage and absorbtion of the sand, the authors present a correlation coefficient and regression equation relating these variables; and also give the standard error of estimate. Denoted as Eq. (2), it is

$$S_{14} = 0.0198A + 0.0723 \pm 0.0516$$

The standard error of estimate, 0.0516, is about four times the magnitude of the corresponding statistic for either Eq. (1) or Eq. (3). This seems rather anomalous, since the magnitude of the standard error of estimate for Eq. (3), which is dependent on those of both Eq. (1) and (2), appears to have been uninfluenced by the latter.

The possibility of error may be checked. The standard error of estimate of each equation is mathematically related to the standard error of the mean of the measurements of drying shrinkage. Thus three independent estimates of the standard error of the mean drying shrinkage (σ_1) can be made, and these should yield practically identical values. The necessary calculations yield the following results:

σ_1 calculated from Eq. (1) = 0.022
σ_1 calculated from Eq. (2) = 0.068
σ_1 calculated from Eq. (3) = 0.022

Two of the results are identical, but that derived from Eq. (2) is three times as large. It then appears that the value 0.0516, given as the standard error of estimate of Eq. (2), is a misprint or that an arithmetical error was made in its calculation. Assuming that the true value of σ_1 is 0.022, the correct value of the standard error of estimate for Eq. (2) is 0.0167.

The previous paragraph is a prelude to suggesting additional implications of the statistics given in the paper.

From the correlation coefficients* and the standard errors of estimate associated with Eq. (1), (2), and (3), it is possible to calculate:

(a) The partial correlation coefficient of drying shrinkage and sand equivalent = 0.679
(b) The partial correlation coefficient of drying shrinkage and absorption = 0.670
(c) The simple correlation coefficient of sand equivalent and absorption = 0.246

The authors conclude from Eq. (3) that "the contribution of the sand to drying shrinkage is dependent to a high degree on sand equivalent and absorption." Items (a) and (b) above show that the degree of association is practically the same for either of the measured attributes of the sand. It follows

*The correlation coefficient associated with Eq. (2) [0.65] is not given in the paper but was obtained from a California Division of Highways report of which the paper is a condensation.

then that, if the measured variation in drying shrinkage is indicative of different degrees of a desirable quality of concrete, prediction of this quality should be based on both attributes of the aggregate.

The simple correlation between sand equivalent and absorption, shown as item (c) above, is statistically significant at the 1 percent risk level. This indicates a stochastic, and possibly a causal dependence of sand equivalent on absorption. The distributing possibility then arises that the indicated dependence of drying shrinkage on sand equivalent may be *only* stochastic and that the former may causally depend primarily on absorbtion.* Certain considerations can be advanced *a priori* which tend to negate the possibility, but the presently available data can neither establish nor disprove it. Regardless of how the question may finally be resolved, its existence illustrates an ever present danger in interpreting results of correlation analysis, *viz.*, that a stochastic correlation does not necessarily demonstrate a causal relationship between the variables examined.

The original paper, considered in conjunction with our discussion, well illustrates the great value of statistical methods for presentation of voluminous data. The authors dealt with 744 test results but published no averages nor individual items of data. They presented only three short equations, three standard errors of estimate, and three correlation coefficients; which could have been accomplished in three printed lines (even the graphs being unnecessary if all prospective readers had been known to be acquainted with the statistical methods and interpretations). Nevertheless, the data were so completely described that we, as discussors, could study these in sufficient detail to detect an error (or misprint) and also to point up other implications of the data.

AUTHORS' CLOSURE

The authors are appreciative of the thorough study that the discussors have made of the paper. Mr. Howard speaks from the standpoint of a producer who has found it necessary to furnish sand to meet a specified sand equivalent test. Although outside of the intended scope of the paper, his comments serve to focus attention on an important problem that arises in the administration of any specification in which a limiting test value is set forth. His reference to a specification minimum of 75 sand equivalent pertains to the "Standard Specifications of the California Division of Highways." These specifications state that in borderline cases, the sand equivalent reported shall be the average of three tests performed on portions of a sample representing the lot in question. Furthermore, individual samples, each from a different lot of sand, may have sand equivalents as low as 70 provided the

*If this be so, use of the sand equivalent test would be an unnecessary step for predicting a quality of concrete which is estimatable from the absorbtion of the aggregate, an attribute which is necessarily measured for another purpose in the routine examination of concrete aggregate.

The fact, that the simple correlation coefficient of sand equivalent and absorption is only about one-third that for drying shrinkage and sand equivalent, does not negate this possibility. The difference in magnitude of the two coefficients could easily result from the fact that in calculation of one of the coefficients each value of sand equivalent is treated as though subject to random errors; whereas, in calculation of the other coefficient each value is treated as an absolute magnitude.

average of three consecutive lots is not less than 75. These specifications, therefore, make considerable allowance for variation in individual results. The use of average results is practicable because the tests can be performed at the work site and can be completed promptly. The details of the test procedure to which Mr. Howard refers will be found in the appendix of a previous paper by one of the authors.*

The precision or reproducibility of the sand equivalent test has its limitations, as do all tests. In Table C, Mr. Howard shows that the standard deviation of a borderline sand is 5, corresponding to a coefficient of variation of 6.7 percent. This result is comparable to a coefficient of variation of 6.0 percent determined by the California Division of Highways. Measures of the precision of a test are significant only when the results used in the computation are limited to those that are shown to be in statistical control. When all tests that could be shown to be influenced by an assignable cause of error were eliminated from consideration in the California study, the coefficient of variation was reduced to 3.2 percent. This value is the true measure of the precision of the test. The study indicated that few, if any, out-of-control results need be obtained if the details of the prescribed method of test are followed faithfully.

It follows that since variance is a factor in the test, the producer of concrete sand cannot expect to receive uniformly acceptable test reports unless he produces his material to have a necessary margin above the minimum specified value. Mr. Howard's estimate of a coefficient of variation of 5 percent appears to be a reasonable working value. The precision of the sand equivalent test compares favorably with that of other tests pertaining to concrete and its constituents, also to asphalt and many other construction materials.

Mr. Tuthill has raised an important question relative to the effect that clays (as measured by the sand equivalent test) have on drying shrinkage apart from that attributable to an increase in unit water content of the concrete. His statement that shrinkage correlates primarily with unit water content appears to be based on tests in which the sand was the same in all cases and the variation in unit water content was the result of varying the consistency and the maximum size of the aggregate. The data obtained by the authors did not cover such variables. The mortar tests, however, permit a study of the relative contribution to shrinkage of unit water content and sand equivalent by a large group of sands of sedimentary, metamorphic, and igneous origin of varied particle shape and limited variation in grading.

The test mortars, nominally 1:2.75 by weight, were adjusted for the specific gravity of the sand so that the ratio of cement to sand by absolute volume was constant. Water was added during mixing as required to produce the selected flow. The net water-cement ratio (corrected for absorption) varied from about 0.38 to about 0.53 by weight. The unit water content increased with increasing water-cement ratio but not strictly in direct proportion;

*Hveem, F. N., "Sand-Equivalent Test for Control of Materials During Construction," *Proceedings*, Highway Research Board, V. 32, 1953, p. 238.

however the theoretical variation from proportionality did not exceed ± 3 percent.

For the entire group of sands the coefficient of correlation between water-cement ratio and drying shrinkage has been computed to be 0.093. The coefficient of correlation between water-cement ratio and sand equivalent has been computed to be 0.128. Neither of these coefficients is significant. If one is willing to disregard the slight deviation from proportionality between water-cement ratio and unit water content, then it can be concluded that there is no significant correlation between unit water content and drying shrinkage or between unit water content and sand equivalent.

For the group of sands considered as whole, it may be concluded that unit water content was determined principally by the characteristics of the sand almost exclusive of the clay content. On the other hand, regardless of the variation in characteristics of the sand, the clay content as measured by the sand equivalent exerted a predominant influence on drying shrinkage.

A possible explanation of the discriminating effect of the sand equivalent test lies in the fact that it differentiates between the different kinds of clay as well as the quantity present.* Equal weights of different clays added to a sand produce greatly different values of sand equivalent.

The discussion of Messrs. Willis and Murray deals mainly with the statistical aspects of the paper and the interpretations to be placed on them. With respect to the possibility of bias in the samples selected for test, it can be stated that the tests for sand equivalent and shrinkage were made on all concrete sands that were received in the headquarters laboratory of the Division of Highways during the course of one year. The frequency curve for sand equivalent of these samples approximated a normal distribution curve.

The criticism of the authors' statement relative to the significance of the coefficient of correlation is well taken. It would have been more precise, in the language of the statistician, to have used the statement suggested by the discussors.

The authors are indebted to these discussors for their detection of an error in the reported value of the standard error of estimate in Eq. (2). The true value computed from the original data is 0.0163 and the error in publication is due to miscalculation on the part of the authors.

The comments on the possible dependence of sand equivalent on absorption are interesting. Computations from the original data show the coefficient of correlation between sand equivalent and absorption to be 0.216 which is not significantly different from the value estimated by the discussors by indirect methods. It is true that this coefficient is significant at the 1 percent level. The discussors then speculate on the possibility of obtaining the desired information on shrinkage characteristics by means of an absorption test rather than the sand equivalent test. Even though the desired control on cleanness could be obtained in this manner, which incidentally would require different specification limits for different sands, there would be a considerable

*See reference on p. 1137.

handicap in elapsed time of completing the test. A sand equivalent test can be completed in 30 min, about the length of time required under favorable circumstances to bring a sample of sand to a saturated, surface-dry condition in preparation for oven drying in the absorption test. Three sand equivalent tests can be completed by one operator in an elapsed time of 40 min. The tests can be performed at the job site in open air if desired.

Finally, the authors wish to point out that they conducted this study, not because of a desire to engage in theoretical research, but because of mounting evidence that older standards of cleanness were not in fact eliminating poorly prepared aggregates. This condition resulted in excessive shrinkage and warping of concrete floors and paving slabs with consequent unsatisfactory performance. The authors believe that the sand equivalent test and its counterpart for coarse aggregate meet a serious need by enabling the engineer to control the cleanness of aggregate promptly during construction.

Discussion of a paper by William Lerch:

Plastic Shrinkage*

By EDWARD A. ABDUN-NUR, F. D. BERESFORD and FRANK A. BLAKEY, M. SPINDEL, LEWIS H. TUTHILL, and AUTHOR

By EDWARD A. ABDUN-NUR†

The author has given a fine exposition of a problem that baffles everyone who has had to place concrete in large flat surfaces, particularly in dry, windy weather. His interpretation of Table 1 is most interesting, and points out the possibilities of drying of concrete surfaces even in moist climates under certain combinations of circumstances.

In connection with the author's discussion of Group 5 of the table, it might be pointed out that the ACI "Recommended Practice for Winter Concreting (ACI 604-56)," if followed, would hold the temperature of the concrete at about 60 F, which would result in negligible evaporation even under the combination of circumstances shown.

The author's fine list of corrective measures is basically an enumeration of good concrete practices. A quick application of membrane curing compound is certainly most helpful regardless of any other measures being used at the time. The writer suggests the use of water-reducing retarders as a worthwhile addition to the fine list of corrective measures given by the author. This class of admixture can minimize shrinkage cracking through the following:

1. Delay the set and thus keep the concrete in a plastic state for a longer period, so that it can adjust to volume changes due to drying without developing cracking.
2. Allow more bleeding so that cracking is minimized through supplying surface water needed for evaporation (except for the very high evaporation rates which occur only occasionally).
3. Permit less water to be used in the mix. This decreases the potential volume change due to drying, and therefore the potential shrinkage cracking.

An added benefit accruing from the use of retarders is the gain in strength through lowered water-cement ratio, thus improving the quality and durability of the concrete.

*ACI JOURNAL, Feb. 1957, *Proc.* V. 53, p. 797. Disc. 53-44 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings*, V. 53.

†Consulting Engineer, Denver, Colo.

By F. D. BERESFORD and FRANK A. BLAKEY*

The review by Mr. Lerch of field observations of cracking in freshly placed concrete is of considerable interest to us.

In most respects the phenomena appear to be identical with what have been discussed in Australia under the name of "presetting or prehardening cracks," but Mr. Lerch does not mention one characteristic feature noted here,[1,2] namely, that the cracks always occur over reinforcement, large pieces of aggregate, or similar obstructions to settlement. The considerable depth of the cracks has been noted here too, and it has also been found that "blind" cracks which do not appear at the surface may be formed within the concrete.

The Division of Building Research is making a laboratory study of prehardening cracks based on the observations which have been recorded,[1,2] and a report will shortly be prepared for publication. Mr. Lerch considers that the main cause of plastic shrinkage or prehardening cracks—if they are the same—is rapid drying of the fresh concrete. Our observations suggest that this is rarely the first cause of a crack, but that it may frequently be the main factor in making the cracks readily visible.

Our laboratory investigations so far have been on slabs made of fluid mixes (compacting factor greater than 0.95), kept in air at 70 F, 50 percent relative humidity. Under these conditions it has been found that if a slab is going to crack, the cracks will appear under the water surface during the period of bleeding. That this is not a laboratory artifact appears to be confirmed by consulting engineers here, who have reported early cracks in floor slabs when there is water on the surface from bleeding or ponding. When the bleeding of the concrete is fast and of fairly short duration, the cracks form early and may disappear about the time the water leaves the surface. Subsequent rapid drying can reopen these cracks.

As the cracks form only a few minutes after concreting, when the bleeding is rapid and most probably the surface is still being worked, they could pass unnoticed or be easily closed off at the top, to be reopened again if subsequent drying is rapid. The writers at present believe that, except in the most extreme cases, cracks which appear to have formed in drying are, in fact, merely reopening.

When the bleeding is long and slow the early cracks are more sharply defined and have less tendency to heal up. Our laboratory work confirms the field observations in Australia that the cracks form either over pieces of aggregate, reinforcement, or some similar obstruction. The general tendency is for mixes to crack over the aggregate when the ratio of coarse to fine aggregate is high, but when the ratio is low the cracks tend to form only over the reinforcement.

As a result of their studies the writers consider that the cracking is associated with differential settlement rather than directly with magnitude or rate of bleeding.

*Technical Officer and Senior Research Officer, respectively, Division of Building Research, Commonwealth Scientific and Industrial Research Organization, Melbourne, Australia.

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2. Mercer, L. B., "Classification of Concrete Cracks," *Commonwealth Engineer* (Melbourne), V. 34, Sept. 1946, pp. 48-49.

By M. SPINDEL*

The importance of shrinkage of concrete and its measurement has been investigated since the beginning of this century in research laboratories and in practice. In the laboratories shrinkage measurements started at first on specimens which had already hardened for a week or 48 hrs and, later, also only 24 hrs.

The results thus obtained were instructive in many directions, but unfortunately they did not tell the whole story.

The lack of knowledge about the shrinkage from the very beginning, i.e., from the time the water was added to the cement, was a great obstacle on projects where it was important to avoid disadvantages due this unknown part of shrinkage, for example, during the construction of bridges and large dams. These disadvantages were noted by the writer, especially after his having made high-early-strength portland cement and concrete in 1915-1916 when bridges of concrete and reinforced concrete made with this cement were loaded 48 hr after the concrete was placed.

To measure the shrinkage of cement from the very beginning, i.e., soon after mixing it with water, the writer tried two different methods. The first one was to fill small glass bottles fully with cement paste and to close them with a watertight stopper in which a capillary tube of glass about 20 in. long was placed containing a short thread of colored liquid which showed by its movement the shrinkage of the cement paste. The results were not published, but two critical points were observed in the movement of the liquid. These points were near the initial setting time, 3 hr after adding water, and the final setting time, 7 hr after adding the water.

In these tests there was no possibility of evaporation of water and the movement of the liquid in the glass tube up to more than 400 mm was due to shrinkage as a result of setting and hardening of the cement paste, i.e., the chemical and physical effect of these without considering the evaporation of water.

As the above method was different from the conditions on the job, the writer at the same time developed another method which also showed the shrinkage distinctly, mainly from the beginning of setting. It was done by measuring microscopically the distance of two pointers put into cement paste or mortar. The length of the two pointers was 100 mm and the shrinkage was measured to 1/1000 mm. The result of dozens of tests showed that the shrinkage started and could be measured immediately after the start of setting and that

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the shrinkage during setting was often greater than that which occurred later, 48 or 24 hr after adding the water according to the usual standard methods.

The writer and his colleagues made good use of these results during job concreting, thus avoiding shrinkage cracks as far as possible. These results have been published by the writer since 1925 in several papers.*

From curves drawn to show shrinkage during setting, it was seen that great differences were obtained with different makes of portland cement, and it may be said that this part of shrinkage could be avoided by adding a special admixture to the cement paste tested. As these microscopical measurements were carried out at different times, exposed to air of different humidity and temperature, the writer together with a manufacturer of testing apparatus designed an apparatus for making these tests under given temperature and humidity which, owing to the war, was not made.

The idea of this discussion is to point out that the shrinkage during setting is important and can be measured according to the method developed by the writer.

By LEWIS H. TUTHILL†

Plastic shrinkage and its causes are among the most elusive of concrete problems and Mr. Lerch has made a valuable contribution in clarifying many of its aspects. The information on rate of evaporation is particularly revealing and there is little doubt but that excessively rapid evaporation is a principal factor in the appearance of plastic shrinkage cracks in many cases.

However, since some might infer from the paper, or at least from the synopsis, that there were no other significant causes of plastic shrinkage, cases are cited in which other factors than excessive drying were evidently the principal causes of plastic shrinkage cracking. At least when these causes were removed, plastic shrinkage cracking no longer appeared. Actually there is probably an existing potential for plastic cracking and a certain degree of such tension in most setting concrete but, fortunately, it develops to the point of checking and cracking only when the combined effect of causes which enhance it is sufficient to reach that point. Thus in varying circumstances any one of several things may be the extra (and often removable) "straw that breaks the camel's back."

In January, 1952, in cool cloudy weather, concrete canal lining by one contractor on an Arizona project at times developed plastic shrinkage cracks. At no time did similar work on the project by another contractor do so and this quite well cancelled out drying as a factor because timing and application of sealing compound for curing was also similar. Although aggregates were different, both were damp from recent rains. Cement was a different brand but did not have false set and was presumably the same on days when plastic shrinkage cracking appeared as when it did not. Continued observation by project engineers detected rather conclusive correlation between the checking

*(Question III, Spindel, Second Congress on Large Dams, Washington, D. C., 1936.)

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and change in character of subgrade material from a sandy soil to a finer silty loam of greater water-absorbing ability despite the usual wetting prior to concrete placement.

In September, 1947, in warm weather, concrete lining being placed in a California canal developed plastic shrinkage cracks consistently, regardless of changes in wind velocity or air temperature. The cracks varied from a few inches to a foot or two in length, were variously spaced from a foot or two apart, and were generally parallel to the center line except along top edges where they were normal. The cement was Type II with a 2 percent limit on SO_3 and it had exhibited false set from the start of the lining operation about the middle of August. In the first few days the contractor had gotten his operations so well organized that the concrete was in place in the lining and the slip-form away from it before serious loss of slump could interfere with placing it. But soon after finishing, even though sealing compound was applied at the earliest possible moment, the plastic shrinkage cracks appeared. Laboratory tests showed that with additional gypsum the cement was free of false set. False set and the plastic shrinkage cracking ceased when the SO_3 content of mill run cement was raised to 2.45 percent. Evidently a property of the cement, not excessive drying, can be the principal factor in causing plastic shrinkage cracking.

In both these cases the cracking was largely stopped by delaying finishing and troweling operations as long as possible. In the Arizona lining a 20-min delay was largely successful. In the California lining a late second troweling usually prevented the cracking. Presumably this later reworking of the surface concrete while still only partially set, relieved the growing tension sufficiently so that it did not build up again enough to pull the concrete apart. This procedure is recommended for any flat work having trouble from appearance of plastic shrinkage cracks, until the principal cause can be eliminated, regardless of what that cause may be.

At the time we were concerned with the case in Arizona and, in view of the cool weather, causes other than drying were suspected, we received a report that in the vicinity of Phoenix plastic shrinkage cracks had appeared in the surface of concrete footings despite the fact that they were flooded almost as soon as the concrete was in place. I would be interested in hearing of other cases where plastic shrinkage cracking clearly developed from principal causes other than excessive drying, especially in concrete covered with water as soon as it was in place.

AUTHOR'S CLOSURE

The number of discussions of the paper indicate a wide interest in plastic shrinkage cracks and provide further valuable information on the problem. Tuthill and Beresford and Blakey cite instances of the occurrence of plastic shrinkage cracks when the concrete is covered with water. Such cracks do occur occasionally. The cause is not clearly known. The author has been

advised that in some cases this type of cracking has been corrected by changing the grading of the sand or by the use of different sand.

Tuthill has suggested that false set or other characteristics of the cement may contribute to the plastic shrinkage cracking. The author has observed two projects where different brands of cement were used and on one of these projects cements of Types I and II were used. Changing the cement did not eliminate the cracking. On one paving project it was observed that the concrete had false set characteristics but there was no evidence of plastic shrinkage cracking even though the weather was clear, dry and hot. On that project the concrete was covered with wet burlap within a very short time after the final finishing.

Beresford and Blakey have observed that the cracks always occur over reinforcement, large pieces of aggregate, or similar obstructions to settlement. The author has noted that on suspended floor slabs, where the reinforcement is close to the top surface, the cracks tend to follow the bars but they may form in other areas. The cracks have been observed on pavements where bars or mesh were not used.

Abdun-Nur suggests the use of water-reducing retarders as a worthwhile addition to the list of corrective measures. Certainly any measure that will reduce the water-cement ratio is worthy of consideration. The use of a retarder may not be desirable in some cases, particularly on floors. It may delay the time when the finish troweling can be accomplished to the extent that cracking will occur prior to the final troweling. Then the use of a fog spray or other corrective measures would be required.

Spindel discussed the contraction of the system cement plus water during hardening. This phenomena is well recognized and has been discussed in previous papers by Hemeon[1] and Powers.[2]

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Discussion of a paper by Jo Dean Morrow and I. M. Viest:

Shear Strength of Reinforced Concrete Frame Members without Web Reinforcement*

By KAMAL N. GHALI, ALAN H. MATTOCK, and AUTHORS

By KAMAL N. GHALI†

The writer would like to comment on the method of calculating the ultimate shear strength of reinforced concrete beams presented by the authors.

In Fig. 11, p. 855, the authors give two diagrams for the probable and assumed strain distributions. The writer agrees that in shear failures there is a rotation around the compressed concrete. In other words, it cannot be assumed that plane sections before bending remain plane after bending. However, the solution for the shear strength of reinforced concrete beams presented by the authors has been arrived at by using some parameters that were obtained from the tests themselves. In this discussion an attempt will be made to present an analytical solution in which no empirical parameters will be needed.

As can be seen from Fig. A, a shear compression failure occurs as follows. First there appear the tensile hair cracks [Fig. A(a)]; on increasing the load one of these cracks begins to incline toward the load point [Fig. A(b)]. If further loads are applied, other inclined cracks may appear until, just before failure of the beam, a flat crack takes place, which joins the load block. It is this crack that causes rupture through crushing of compressed concrete.

For beams with long shear spans the above-mentioned procedure takes place at one time. This type of failure was designated by the authors as "diagonal tension failure." The writer holds that the beam still fails by crushing of concrete; this can be seen from Beam B56B4, Fig. 4, p. 846. This is to say that once the inclined cracks have appeared, the redistribution of stresses has taken place, the compressive strength of concrete is exceeded, and the beam fails. Thus the capacity of such beams corresponds to the load that causes "initial diagonal tension cracks." But the bending moment caused by this load should still be smaller than the flexural failure load of the beam.

If the bending moment caused by the "initial diagonal tension cracking load" is greater than the flexural capacity of the beam, there will be a flexural failure. This is the case for long shear spans, where the flexural capacity of the beam is attained before inclined shear cracks develop.

*ACI Journal, Mar. 1957, *Proc.* V. 53, p. 833. Disc. 53-47 is a part of copyrighted Journal of the American Concrete Institute, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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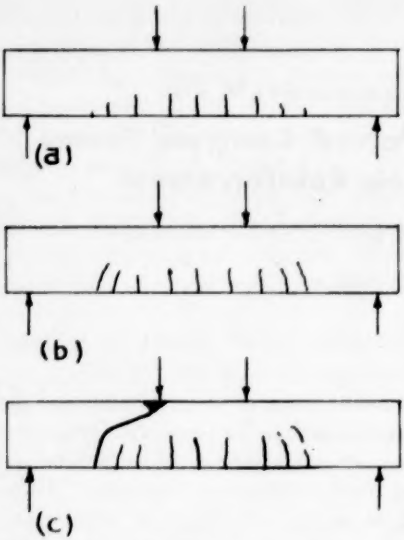

Fig. A—Shear failure progress

The writer has tried to develop an analytical expression for shear compression failures. In Fig. A(b), it can be seen that the distribution of stresses at the critical section can be assumed to be linear. This linear distribution holds correct until the flat crack has formed. After the formation of this flat crack straight line distribution of stresses can not be assumed.

Since the formation of this flat crack indicates impending failure of the beam, the writer holds that the stress distribution at the moment of failure can be represented by the maximum possible straight-line distribution, i.e., by a straight-line distribution in which the maximum concrete stress is to be taken equal to f_c'. In other words, it will be assumed that the existence of shear stresses does not allow the concrete to develop its plastic compressive resistance. This assumption seems to be justified by the following two experimental facts: (a) in shear failures the steel stresses remain generally below the elastic limit; (b) shear failures occur without excessive deformations, the rupture always being sudden.

According to this assumption the steel stress at the moment of rupture is

$$f_s = n f_c' \left(\frac{1}{k} - 1\right) \quad \text{(a)}$$

This is the well-known formula of the classical theory, when the concrete stress is equal to f_c', k being the ratio of the distance of the neutral axis from the extreme compressed fibers, divided by the depth of the beam.

Thus total tensile force is

$$F_t = p\, b\, d\, n\, f_c' \left(\frac{1}{k} - 1\right) \quad \text{(b)}$$

and the ultimate shear moment causing failure is

$$M_u = p\, b\, j\, d^2\, n\, f_c' \left(\frac{1}{k} - 1\right) \quad \text{(c)}$$

Eq. (c) can now be checked against the experimental results. However, it has to be kept in mind that the shear failure should satisfy the two following conditions: (a) the shear stress must be sufficient to produce a diagonal tension crack; (b) the bending moment must be sufficient to produce the crushing of concrete. These two conditions may be expressed in one equation through the following procedure. Taking

$$v_u = \frac{V}{b\,j\,d}$$

if we multiply by a/a, we have

$$v_u = \frac{M}{a\,b\,j\,d}$$

But $M/jd = F_t$. Thus

$$v_u = \frac{F_t}{a\,b} \qquad \text{(d)}$$

Substituting the value of F_t as obtained in Eq. (b):

$$v_u = p\,\frac{d}{a}\,n\,f_c'\left(\frac{1}{k} - 1\right) \qquad \text{(e)}$$

In this equation v_u is the ultimate shear stress calculated according to the equation (8/7) (P_u/bd). Where

p = percentage of tensile reinforcement

d = depth of the beam

a = shear span

$n = \frac{E_s}{E_c}$

f_c' = compressive strength of concrete cylinders

$k = \frac{y_1}{d}$ ratio of the distance of the neutral axis divided by the effective depth of the beam

Eq. (e) then represents the ultimate shear stress of rectangular reinforced concrete beams. For the so-called "shear compression failure" this equation shall be seen to satisfy the experimental results. For long shear spans it gives a value of $v_u < v_c$. If this is the case, the equation cannot be applied.

On the other hand, since Eq. (e) has been derived from Eq. (b) (representing the steel tensile force F_t) it follows that the value of f_s should not be greater than the yield strength of steel. If this is the case the ultimate shear stress v_u has to be computed as follows:

$$v_u = \frac{F_t}{a\,b}$$

$$v_u = \frac{pbd\,f_y}{ab} \qquad \text{(f)}$$

TABLE A—SHEAR COMPRESSION FAILURES BEFORE YIELDING OF REINFORCEMENT

Specimen	p, percent	f_c', psi	n*	k	v_u calc.†	v_u test	Ratio $\frac{v_u \text{ test}}{v_u \text{ calc.}}$	Ratio‡ $\frac{M_{ut} \text{ test}}{M_{ut} \text{ calc.}}$
B14B2	1.85	2120	9.25	0.44	485	543	1.12	0.89
E2	0.57	1840	9.92	0.286	293	403	1.38	1.14
A4	2.50	3270	7.44	0.454	757	767	1.01	0.91
B4	1.85	3820	6.88	0.394	806	760	0.94	0.91
E4	1.24	4190	6.57	0.333	715	753	1.05	1.04
A6	3.83	6590	5.24	0.465	1545	1375	0.89	0.97
B6	1.85	6780	5.17	0.353	1240	1170	0.94	1.05
B21B2	1.86	2010	9.5	0.445	314	353	1.12	0.88
A4	2.46	4320	6.47	0.428	645	772	1.20	1.16
B4	1.85	3930	6.7	0.391	534	585	1.09	1.04
A6	3.83	6570	5.26	0.466	1002	888	0.88	0.94
B6	1.82	6600	5.23	0.353	818	838	1.02	1.11
B28B2	1.88	2130	9.22	0.443	240	300	1.25	1.12
E2	0.57	1990	9.55	0.282	147	187	1.27	1.13
A4	2.46	3990	6.73	0.434	453	475	1.05	1.12
B4	1.85	4690	6.21	0.379	462	379	0.82	0.92
E4	1.24	4800	6.15	0.324	402	394	0.98	1.15
A6	3.83	6840	5.14	0.461	793	506	0.64	0.81
B6	1.85	6360	5.34	0.359	588	476	0.81	1.02
B40B4	1.85	5040	6.0	0.375	342	234	0.68	0.88

*$n = E_s/E_c$; E_s was taken as 2,100,000 kg per sq cm; E_c was taken as $18{,}600\sqrt{f_c'}$ (kg per sq cm), with f_c' taken from Table A and converted to kg per sq cm (multiplied by 0.0703).
†v_u calc. is calculated according to Eq. (e).
‡Values according to the authors' equations (Table 8, p. 859).

Eq. (f) represents the ultimate shear stress of beams failing in shear after the yielding of reinforcement. This equation is somewhat "on the safe side." An examination of the yield point load P_y and the failure load P_u (Table 6, p. 842) shows that the error is always within a reasonable percentage.

However, for very long shear spans and/or for low values of p, the value of v_u as given by Eq. (f) might be less than v_c; in such a case the inclined cracks cannot develop and the beam fails in flexure.

It remains now to check Eq. (e) and (f) against the experimental results of the authors. Shear compression failures before yielding of reinforcement are included in Table A. As can be seen from this table, Eq. (e) goes well with the experimental results.

Shear compression failures after yielding of reinforcement are included in Table B. As stated before, Eq. (f) is always on the safe side, but it is the writer's point of view that if the yield point load P_y had been sustained for a long time the beams might have finally failed under this load.

Diagonal tension failures are included in Table C. In Table C the values of v_u calc. are computed according to Eq. (e). It can be seen that this equation goes equally well with the experimental results. This might be due to the fact that diagonal tension failures can in some respects still be considered as shear compressoin failures. However, more tests will be needed before it can be concluded that Eq. (e) can be applied fairly well.

TABLE B—SHEAR COMPRESSION FAILURES AFTER YIELDING OF REINFORCEMENT

Specimen	p, percent	f_y, ksi	F_c, kips	v_u calc.*	v_u test	Ratio $\frac{v_u \text{ test}}{v_u \text{ calc.}}$	Ratio† $\frac{M_{st} \text{ test}}{M_{st} \text{ calc.}}$
B21E2	0.57	67.2	68.2	271	306	1.13	1.11
E4	1.24	62.4	134.2	534	610	1.14	1.16
E4R	1.24	60.4	130.0	516	643	1.24	1.17
F4	1.17	66.2	128.0	507	688	1.36	1.22
B56E2	0.58	67.2	68.0	101	117.7	1.16	1.11

*v_u calc. is computed according to Eq. (f).
†Values according to the authors' equations (Table 8, p. 859).

Rectangular beams with web reinforcement

The above-mentioned method of analysis can be extended to cover the case of rectangular reinforced concrete beams with web reinforcement. In this respect Eq. (e) can be extended to have the form:

$$v_u = p \frac{d}{a} n f_c' \left(\frac{1}{k} - 1 \right) + 0.8 r f_y \quad \text{(g)}$$

where: r = percentage of web reinforcement
f_y = yield point strength of stirrups

The multiplication of $r f_y$ by the factor 0.8 is due to the fact that at the moment of failure not all stirrups arrive at the yield point strength.

It might be added that Eq. (e), (f), and (g) go well with all the experimental data to be found in the literature, though it is beyond the scope of this discussion to include the analysis of all these results.

Eq. (e), (f), and (g) as developed can by no means be claimed to be more reliable than the authors' equations, but they might have the advantage of being simple and free from empirical parameters.

TABLE C—DIAGONAL TENSION FAILURES

Specimen	p, percent	f_c', psi	n	k	v_u calc.*	v_u test	Ratio $\frac{v_u \text{ test}}{v_u \text{ calc.}}$	Ratio† $\frac{v_u \text{ test}}{v_u \text{ calc.}}$
B56B2	1.85	2130	9.22	0.44	122.8	142	1.16	1.00
A4	2.41	3620	7.07	0.44	209	199	0.955	1.02
B4	1.85	3950	6.66	0.389	201	182	0.905	0.99
E4	1.24	4120	6.65	0.335	178	160.5	0.902	0.97
B56A6	3.79	5780	5.6	0.483	327	272	0.835	1.04
B6	1.83	6630	5.12	0.351	307	199	0.650	0.92
B70B2	1.86	2370	8.75	0.432	106	132.7	1.25	0.93
A4	2.46	3950	6.77	0.435	180	193.5	1.07	1.03
A6	3.83	6520	5.26	0.466	308	274	0.89	1.07
B84B4	1.88	3950	6.78	0.395	134	166	1.24	1.02
B113B4	1.86	4730	7.4	0.407	123.7	156	1.26	1.00

*v_u calc. is computed according to Eq. (e).
†Values according to the authors' equations (Table 7, p. 853).

By ALAN H. MATTOCK*

Variations in mode of shear failure with change of shear span to effective depth ratio, similar to those reported in the above paper, have been observed during tests at Imperial College, University of London.

Tests have been carried out on rectangular reinforced concrete beams without web reinforcement, over-all dimensions 4 x 9 in., effective depth 7½ in. The beams were tested at 28 days, the concrete cube crushing strength being approximately 3000 psi. A single point load was applied through a roller resting on a ½ in. thick steel plate measuring 4 x 2 in. The beams were supported on similar rollers and plates. The shear span was taken as the distance from the center line of the loading roller to the center line of the supporting roller.

The influence of variation of a/d ratio was studied for two percentages of reinforcement, 3.93 and 1.23 percent. The former corresponds to the maximum percentage of longitudinal reinforcement permitted under the present British "Code of Practice;" the latter is almost exactly equal to the "economic" percentage of reinforcement corresponding to concrete and steel stresses of 1000 and 18,000 psi, respectively, using the straight line design theory. (The 1000 psi design stress is permitted for concrete having a cube strength of 3000 psi.) The results so far obtained are summarized in Fig. B in which the bending moment at failure is plotted against the ratio of shear span to effective depth. The bending moments actually obtained have been adjusted for concrete strength to give the value corresponding to a concrete cube strength of 3000 psi.

It is evident that the bending moment at shear failure is not independent of the a/d ratio as had been previously suggested, but in fact varies considerably and has a clearly defined peak value. It was noted that for values of a/d greater than that at which the peak moment occurred, only one true diagonal tension crack formed, while for smaller values of a/d two or more diagonal tension cracks appeared before failure. The compression failure zone was unlike that occurring in a flexural failure, being elongated in the direction of a line joining support and load, and having a width at the compression face equal to that of the loading plate. The length of the zone of crushing increased as the ratio a/d decreased, until for very small values of a/d the compression zone at failure extended to the level of the steel.

For short spans it appears, therefore, that final failure is due to crushing of a concrete strut joining load and support. The effective width of the strut appears to be dependent on the width of the loading plate, the depth of the neutral axis (and therefore of the percentage of longitudinal reinforcement and a/d ratio), and on the angle to the horizontal made by a line joining the center of the loading plate to the center of the bearing plate. Calculation of

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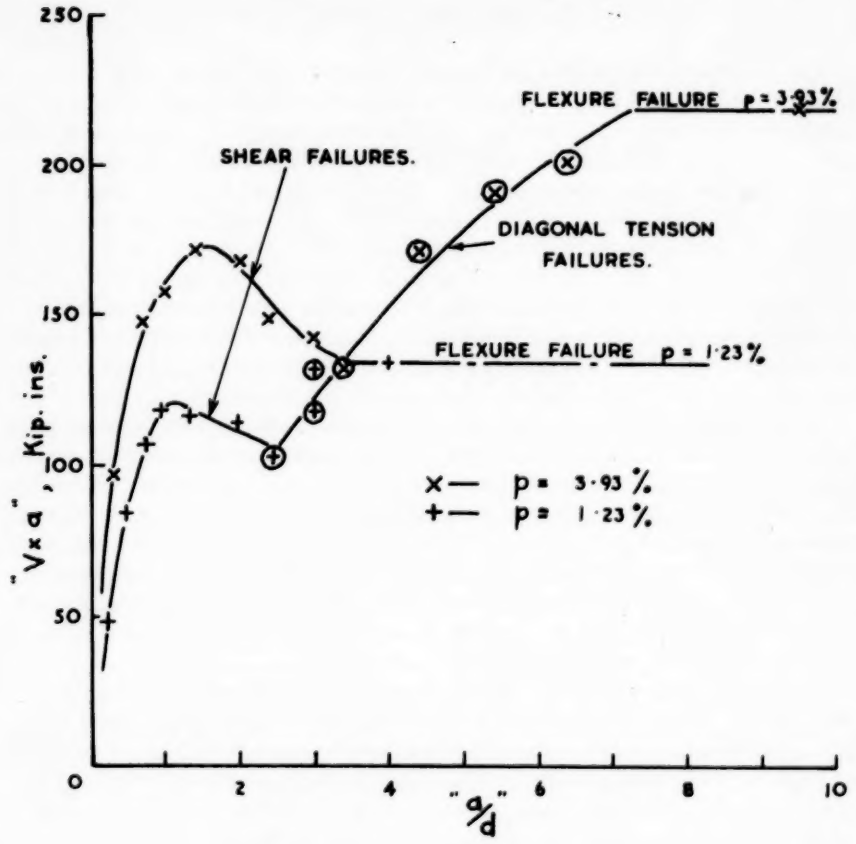

Fig. B—Variation in bending moment at failure

shear strength as the vertical component of an inclined strut with cross section dependent on the factors outlined above leads to a curve for moment at failure versus a/d similar to those drawn on Fig. B.

As in the tests described by Messrs. Morrow and Viest, it was found that for diagonal tension failures the shear at failure decreased almost linearly with increase in a/d ratio. Over the whole range of spans in which diagonal tension failures occurred for beams having 3.93 percent reinforcement, there was a total reduction in shear strength of about 23 percent.

The results obtained from tests on simple prismatic reinforced concrete beams at Imperial College are therefore complementary in every way to those obtained for reinforced concrete stub beams and knee frames by Messrs. Morrow and Viest at the University of Illinois.

AUTHORS' CLOSURE

The investigation reported by the authors represents but one step in current studies of the effect of shear on the behavior and strength of reinforced concrete beams without web reinforcement. The contribution by Dr. Mattock extends the available information into the range of extremely small a/d ratios.

Dr. Mattock's description of the observed modes of failure is particularly interesting. It suggests that the failure of the compression zone of beams with short a/d ratios is similar to a column failure. The authors' tests of stub beams with 14-in. shear span exhibited a similar behavior. In beams with longer a/d ratios, however, the failure of the compression zone was often similar to a flexural compression failure. These phenomenological differences can be easily explained if, after the formation of diagonal tension cracks, a beam without web reinforcement is considered a tied arch.

It was pointed out in the concluding remarks of the paper that a diagonal tension crack in a beam without web reinforcement widens rapidly and often can lead to splitting along the tension reinforcement. For these reasons, *the diagonal tension cracking load must be considered in the design as the ultimate capacity of all beams without web reinforcement.* Thus the shear-moment equations for beams without web reinforcement are unimportant from the design viewpoint. They are useful only as a means for explaining the behavior of the test specimens and as a step in the studies of the behavior and strength of beams with web reinforcement.

Mr. Ghali presented a shear-moment equation based on the assumption of straight-line stress distribution and on limiting concrete stress equal to f_c'. The experimental evidence presented in this and other recent papers certainly contains no justification for such an approach as an explanation of the behavior. And, as has been pointed out above, a shear-moment equation for beams without web reinforcement is *a priori* of no use to the designer.

Discussion of a paper by Herman G. Protze:

Structural Refractory Concrete*

By OLIVER G. JULIAN and FRED H. LINDQUIST

By OLIVER G. JULIAN†

Although the marked initial weakening and retrogression in strength of refractory concrete which are bound to occur if the temperature of hydration is allowed to exceed approximately 100 F was pointed out by Davey[2] in 1933 and by Lea and Desch in 1935, (see author's References 1 and 2) these facts seem to have been ignored by many. The importance of keeping the concrete cool during the hydration period is again pointed out but perhaps not sufficiently emphasized by Mr. Protze. By some, the preparatory work, constant care, and degree of supervision necessary to prevent the heat of hydration exceeding 100 F are so great as to be considered impracticable, if not quite impossible. However, my limited experience in this field indicates that it can be done provided the "will to do" is present.

By FRED H. LINDQUIST‡

As field structural engineer on jet engine test cell construction in 1951 and 1952, the writer was involved with the fabrication of precast exhaust stack sections of refractory concrete. The ingredients were American high-alumina cement and expanded shale aggregate in the ratio 1:2.75:3.0 by moist loose volume. The precast stack sections, weighing from 9 to 16 tons each, were of 8 in. wall thickness and were fabricated in accordance with a detailed specification which established 100 F as the maximum desirable temperature of the concrete mass during hydration. The procedures followed to satisfy this condition included those outlined in Mr. Protze's paper. In addition the contractor erected a steel-framed structure to house the casting operation with sheds for covered storage of aggregate. The aggregate was kept cool and at optimum moisture content by fog sprays. The steel forms were precooled and maintained cool during and after casting by water sprays. The mixer was cleaned after each batch and cooled by water spray immediately before introducing new material.

Fig. A records the values of maximum temperatures of hydration experienced in 206 concrete sections in the form of a histogram and probability curve. The ordinates of the histogram shown as cells drawn in dots represent the

*ACI JOURNAL, Mar. 1957, *Proc.* V. 53, p. 871. Disc. 53-48 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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relative frequency of maximum temperatures occurring within the cell widths indicated by the abscissas at their bases. To have a standard for comparison, the width of these cells has been arbitrarily chosen as one probable deviation according to the Gauss-Laplace normal distribution. The figures shown in circles indicate the number of temperature readings pertaining to each cell of the histogram. The curve drawn in solid lines represents the cumulative frequency or probability of occurrence. The ordinates pertaining to this curve indicate: (1) to the left of the median, the percentage of total number of maximum temperatures below the value indicated by the corresponding abscissa, and (2) to the right of the median, the percentage of total number of maximum temperatures above the value indicated by the corresponding

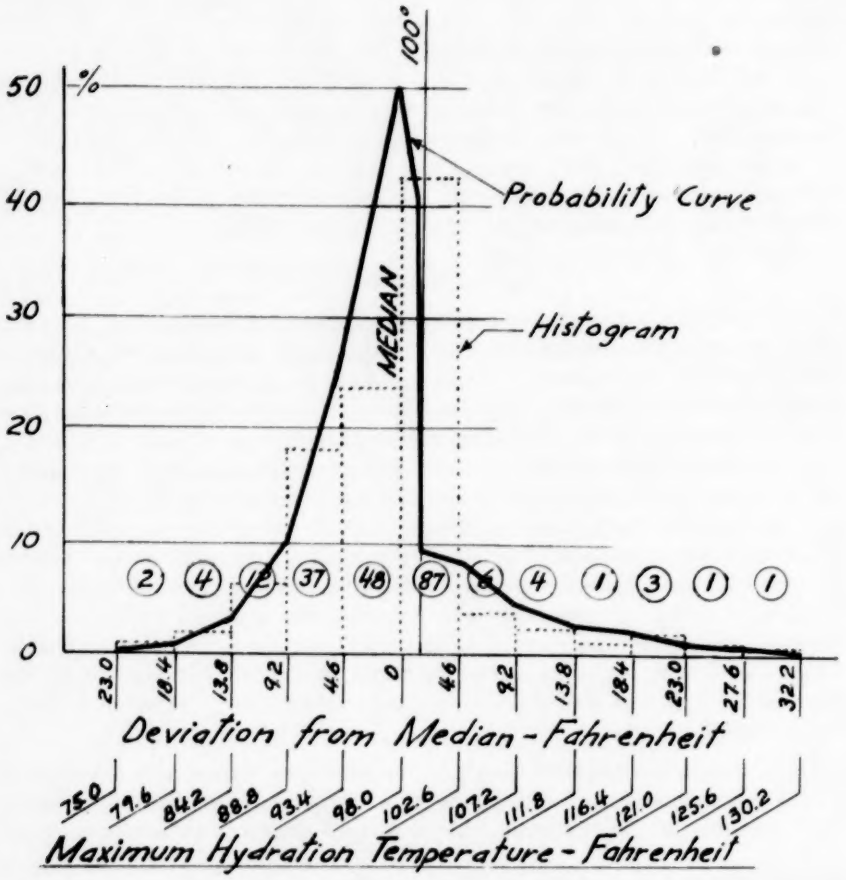

Fig. A—Histogram and probability curve for temperatures of hydration in precast structural refractory concrete

abscissa. The sharp break in the probability curve to the right of the median is due to but three maximum temperature readings being between 100 F and 102.6 F. It will be noted that only 9.2 percent of the total number of maximum readings were above 100 F. The higher temperatures were due to assignable causes which were remedied by revision to the cooling water spray set up whenever unsatisfactory conditions were revealed by the thermometers.

The measure of success achieved in this operation was due to constant supervision and, in great part, to the acceptance by the contractor's personnel of the premise that temperature control is of prime importance in the manufacture of structural refractory concrete, and to their willingness to do what was required to obtain this control.

Discussion of a paper by Norman B. Green:

Design of Floating Slab Foundation*

By JAMES CHINN, STANLEY JAMES GOLDSTEIN, A. MYSTKOWSKI, and AUTHOR

By JAMES CHINN†

Mr. Green is to be commended for devising a new and imaginative method for the design of floating slab foundations. Upon first reading Mr. Green's paper, the writer was skeptical about some of the assumptions made. An examination of the writer's objections, however, proved them to be of more theoretical than practical importance.

The author apparently objects to the beam-on-elastic-foundation solution to his problem because such a solution assumes the foundation can exert downward as well as upward forces upon the beam. This objection is certainly valid for a concentrated load applied to a weightless beam lying atop a bed of closely spaced springs; however, a beam having weight would initially compress the springs, and the beam-on-elastic-foundation solution would be valid as long as the downward reaction required of the springs by concentrated load did not exceed the initial compression.

This argument appeared to apply to the author's example problem, until the writer remembered that earth, unlike a spring, cannot rebound after compression, so there will no longer be a reaction between slab and ground wherever the slab lifts up even the least amount upon application of concentrated load.

The assumptions that no moment exists at point C, Fig. 4, and that the tangent to the elastic curve is horizontal at this point seem, at first, to be a bit far-fetched. The beam-on-elastic-foundation solution involves an elastic curve which oscillates with decreasing amplitude about the zero line. At the point in this solution corresponding to point C, neither the moment nor the tangent to the elastic curve is zero. It would seem to the writer that the solution to Mr. Green's problem should also oscillate with decreasing amplitude and produce moment and slope at point C. When one considers the rate at which the magnitude of the moment and slope decrease, however, it is not unreasonable for them to be so small as to be negligible within one or two oscillations.

One rather serious error does exist in Mr. Green's paper, and that is the assumption that the maximum moment exists at point B, Fig. 4. Taking a

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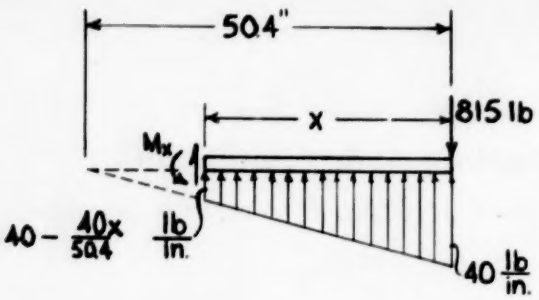

Fig. A—Free body diagram of portion of slab

free body of a portion of the slab to the right of point B (Fig. A) the moment for the example problem is found to be

$$M_x = 815X - 20X^2 + 0.132X^3$$

and the shear is

$$V_x = 815 - 40X + 0.397X^2$$

The moment is maximum where the shear is zero, or at $x = 28.1$ in. The value of the maximum moment is 10,040 in.-lb per ft instead of the 6750 found by Mr. Green. This will increase the steel area required for negative moment from 0.096 to 0.143 sq in. per ft.

The writer believes that to complete the free body of the portion of slab to the right of point B, Fig. 4, the slab weight acting downward is required together with an equal and opposite subgrade reaction acting upward. This has no effect on shears or moments, but it makes the maximum soil pressure 555 psf rather than 480.

In the writer's opinion, Mr. Green's method gives usable results for design purposes, although it involves some assumptions which are not exact. The writer feels, too, that the beam-on-elastic-foundation solution also gives usable results despite its shortcomings, and he finds some experimental evidence to support this.[1] It would be interesting, then, to compare results by the two methods.

For the beam-on-elastic-foundation, the following equations apply.[2]

$$y = \frac{Pe^{-\beta x}}{2\beta^3 EI}\cos \beta x; \quad \text{where } \beta = \sqrt{\frac{k}{4EI}}; \quad k = \text{modulus of subgrade reaction} = m/12$$

$$\frac{dy}{dx} = -\frac{Pe^{-\beta x}}{2\beta^2 EI}(\cos \beta x + \sin \beta x)$$

$$M = -\frac{Pe^{-\beta x}}{\beta\, EI}\sin \beta x$$

For the author's example:

k = 100 lb per cu in.; E = 2,000,000 psi; $I = \frac{6^3}{12}$ = 18 in.^{4} per in.

P = 815 lb per ft

$\beta = \sqrt[4]{\frac{k}{4EI}} = \frac{1}{20\sqrt{3}}$ in.$^{-1}$

y_{max} = 0.00392 in. at $x = 0$

Maximum soil pressure due to $P = ky_{max} = 100\,(0.00392)\,(144) = 565$ psf versus 480 psf
Distance corresponding to a, Fig. 4, = 54.4 in. versus 50.4 in.
M_{max} = 9100 in.-lb per ft versus 10,040 in.-lb per ft

A useful table for numerically evaluating y, dy/dx, and M_{max} can be found in Reference 2. The maximum moments by this method are found to be relatively insensitive to variations in k and E just as they are by Mr. Green's method.

Highway and airport designers have used the elastic foundation solutions for slabs for some time. Reference 3 will be found extremely useful for handling concentrated loads on slabs.

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By STANLEY JAMES GOLDSTEIN*

This is a deceptively simple subject, as basic and as common a building design problem as man ever faced. It is a step forward to have Mr. Green's presentation of a rational method of calculating the forces involved in this configuration of on-grade shapes. But, the configuration itself is open to serious challenge as far as applicability and practicability in many circumstances, irrespective of geographical location of site.

The analysis presented by Mr. Green does not completely integrate the practical and theoretical considerations involved in placing soil-bearing structural members near the finish grade. For example, the analysis does not take into account the sensitivity of the soil to loss or gain of moisture, nor of its susceptibility to frost action (which is related to moisture content, soil type, and grain-size distribution). It does not take into account the not

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unusual necessity for thermal separation of slab and foundation; likewise, of noncapillary separation of structural members from the subgrade material (i.e., gravel or crushed stone structural blanket and noncapillary layer placed as a base course); likewise, of the effect of separating base course from slab by a relatively smooth moisture barrier membrane, which then acts to reduce or eliminate any intrinsic interlock between slab and base materials. Normal shrinkage might result in considerable cracking in any 28 ft wide slab, and this could be of considerable importance in the assumptions basic to the concept of the "width of slab activation."

Most one-story buildings built directly on the ground are so sited as to be elevated somewhat above the immediate surroundings, for good drainage away. Many times this requires grading the area to be built on, such that shallow exterior perimeter beams or haunched slabs bear upon disturbed soil, or, at least, soil of questionable compaction and density. At the minimum, 6 to 8 in. of topsoil must be stripped from the typical site before grading operations are commenced. Referring to Fig. 1, the finish floor elevation can be raised no more than 1 ft (assuming a perfectly level site) above the bottom of the topsoil layer (to allow the sloping of the finish grade away from the immediate vicinity of the building), else the bottom of the haunched member would then be resting on fill material, rather than undisturbed soil.

Poor workmanship in compacting backfill of any kind or description is one of the most common abuses in building construction practices today. Despite the low soil pressures involved here (in fact, because of their low magnitude), and despite Mr. Green's demonstration of the low sensitivity of his formulas to changes in soil characteristics, it would appear that the configuration of shallow beams is particularly susceptible to shrinkage and settlement due to poor grading, localized erosion due to poor drainage circumstances, as well as all of the other soils matters referred to above. These points may be of only minor importance, when scrutinized by theoretical calculations alone, but could be of interest to the owners of buildings, if cracked floors and erupting floor finishes result. This critic is one of the first to question the economic necessity for carrying costly and bulky foundations to considerable depths below the finish grade. Yet, it would appear that the other extreme, of using shallow edge thickenings of the slab, may be far from sufficient to insure a permanent and stable structure under the more typical construction conditions.

The question of anchorage into a nonsensitive and/or undisturbed depth of the subgrade, versus a complete floating action, needs painstaking analysis, both theoretical and practical: not by consideration of structural loads alone, but by an all-embracing evaluation of the multitude of natural, physical, and human forces at work.

By A. MYSTKOWSKI*

The author proposed an approximate solution for a slab-on-elastic-foundation in the case of two concentrated loads acting on its edges.

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It may be interesting to see how much more or less complicated and time consuming it would be to find the theoretically correct solution for this particular case.

To do so, it is not necessary to spend time for solving the basic equation:

$$EI = \frac{d^4y}{dx^4} = -my \qquad \text{(I)}$$

In Timoshenko's *Strength of Materials*, Part II, Chapter I, paragraph 2, one can find the direct answer for deflection Y_p under the load acting on the edge of the semi-infinite beam, as obtained from Eq. (I).

$$Y_p = \frac{1}{2\beta^3 EI}(P - M_o) \qquad \text{(II)}$$

in which expression one should substitute $M_o = 0$ because there is no bending moment acting upon the end of the slab.

Substituting further in Eq. (II) the values

$$E = 2 \times 10^6; \; I = 216$$

$$\beta = \sqrt[4]{\frac{m}{4EI}} = \sqrt[4]{\frac{1200}{4 \times 2 \times 10^6 \times 216}} = 0.0288$$

and keeping in mind that the soil pressure $p = my$, one obtains:

$$p_{max} = my_p = \frac{1200 \times 815}{2 \times 0.0288^3 \times 2 \times 10^6 \times 216} = 47.5 \text{ lb per in.}$$

or 570 lb per sq ft from the concentrated load only. Adding the weight of the slab, the maximum soil pressure is 570 + 75 = 645 lb per sq ft.

The evaluation of bending moment is more time consuming. However, once the maximum soil pressure under the loaded edge is found, a simple approximate method may be used. Here we may assume triangular distribution of soil pressure and write

$$\frac{ap_{max}}{2} = P; \; a = \frac{2 \times 815}{47.5} = 34.3 \text{ in.}$$

The centroid of this triangle is at a distance 34.3/3 = 11.4 in. from the edge (the point of load application). Hence the bending moment = 11.4 × 815 = 9300 in.-lb. The actual bending moment, computed accurately, is = 9150 in.-lb and occurs at about 28 in. from the slab edge.

The method proposed by the author does not seem to simplify the computation to the extent that would justify the considerable inaccuracy in the final result. Using Eq. (II), it is possible to obtain the proper answer much more readily than by the proposed method.

AUTHOR'S CLOSURE

The discussion by Professor Chinn brings out clearly the unrealistic assumption concerning the action of the soil, on which the beam-on-elastic-foundation solution is based. It is therefore apparent that this solution is not "theoretically correct" and cannot be used as a standard of accuracy for this general problem, as has been done by Mr. Mystkowski. It is difficult to estimate how much error is likely to be involved by the assumption that the soil exerts a downward reaction tending to prevent uplift of the slab, but it does appear that the calculated moments will thereby be reduced. This downward reaction is analogous to a live load on the slab, which reduces the moment, as mentioned in the paper. On the other hand it is believed that the approximations in the author's analysis lead to higher moments and are therefore on the safe side.

It is interesting that the maximum bending moment for the example of the paper, as calculated by Messrs. Chinn and Mystkowski using the beam-on-elastic-foundation analysis, agrees so well with that calculated from the author's analysis. This analysis gives a larger moment, as would be expected since the approximations are on the safe side. This rather close agreement is probably due to the fact that the maximum uplift of the slab for this particular case is relatively small. For the same identical conditions, except an 8-in. slab instead of a 6-in. slab, the author has calculated the maximum uplift in the length l to be 0.000415 in., while the depression e at the edge is 0.0267 in. Obviously the assumed downward reactions for this case, under the beam-on-elastic-foundation theory, would be small and would have little effect on the maximum moment. It is likely that in cases for which the uplift is relatively larger, the results from the two methods would not be in such close agreement and the beam-on-elastic-foundation analysis might be considerably in error.

Professor Chinn has pointed out an important fact, that the maximum moment in the slab is not at point B of Fig. 4, but is at a point 22.3 in. therefrom, where the shear passes through zero. This larger moment should have been used in calculating the slab reinforcing in the example.

Mr. Goldstein has posed some important practical questions concerning the use of this type of foundation. The author will answer some of these questions in accordance with experience on school buildings in the Los Angeles area.

With regard to the question of adapting a level foundation slab to a sloping site, this simply results in all or a portion of the slab resting on fill. Where the finished grade is considerably below the floor, the fill is either sloped up to the edge beam or the edge beam is deepened by stepping down as required. In

the case of relatively long buildings, one or more transverse vertical steps in the floor slab have been used and present no particular design problem. The fill is generally selected material compacted to a minimum of 90 percent maximum density, in accordance with the American Association of Highway Officials method. It is easy to secure this compaction, which will generally give adequate bearing value for the relatively low soil pressures. In some cases fill placement has been controlled by density tests made in the field by soils engineers.

Certain precautions must be taken when this type of foundation is used on a soil subject to volume change with change in moisture content, a type of soil that is known locally as adobe and which is quite prevalent in certain areas. The change in volume is ordinarily due to the penetration of surface moisture, either from rain or irrigation. Where the slab is surrounded by concrete or asphaltic concrete walks or yard paving, no trouble from this cause need be anticipated. If the paving is not continuous, a strip of it around the edge of the slab and sloping away from it, will prevent penetration of water under the slab. It has been found that 3 or 4 ft width of strip is adequate. Planting requiring irrigation should not be placed adjacent to the slab, or it should be placed in watertight planters. Such a strip of paving also may be used to prevent possible erosion at the grade beam, if this appears likely for friable soils. In the Los Angeles area frost action does not occur.

Some trouble has been experienced with the proper placement of slab reinforcing and concrete during the rainy season, when the soil is of a type that becomes soft and sticky when wet. In such cases a 2 or 3 in. thick layer of granular material with some binder, has been placed to serve as a stable working surface.

In no case has a gravel or crushed stone blanket or base course been used under the slabs. The soil type at different sites varies widely and includes adobe, fine sand, silty sand, and coarser sand and gravel. The water table was nearly at ground surface in some places. No trouble has been experienced by penetration of moisture from beneath the slab.

With regard to cracks in the slabs due to shrinkage or temperature effects, several buildings with this type of foundation have been inspected as long as 2 years after completion, and there has been found to be a noteworthy absence of cracks. These slabs are better in this respect than the conventional 4 in. thick slab.

Discussion of a paper by Grant Bloodgood and Lewis H. Tuthill:

Responsibilities of an Inspector*

By MARVIN J. HAWKINS, J. W. KELLY, and JOSEPH J. WADDELL

By MARVIN J. HAWKINS†

The paper "Responsibilities of an Inspector" is one that will interest all construction personnel. It is gratifying to see articles of this type presented, to clarify and make known to all concerned the responsible position of the inspector.

Personally knowing the authors of this paper and their experience background, I feel certain we could not find individuals more qualified to advise upon the subject of inspection. Also, having been in attendance at the Denver training sessions when Mr. Bloodgood presented the paper, it was possible to observe first hand the interest displayed by the attending field men. Of particular note were the remarks about supervisory support of inspector actions and the benefit to be gained from open discussions of job requirements to establish uniform inspection standards for all inspectors.

From personal contact with inspectors during the past 33 years, certain opinions as to inspector actions on the job have been formulated. A few of these actual job experiences will be reviewed in my discussion.

It should be noted that the authors have presented their paper in three sections and the sections will be reviewed separately.

First, "Inspector's Place in the Organization" defines the usual line of authority followed on typical construction operations. A point mentioned as one responsibility of the inspector that should be stressed is to follow approved standards of inspection. The job specifications furnish the requirements and when any doubt exists as to interpretation of the requirements the inspector is supposed to consult his chief inspector or superior. Chief inspectors are experienced men who generally work closely with the inspectors to establish uniform "job-wide" inspection standards. These inspection standards will eliminate unnecessary details and direct attention toward important items that will result in lower construction costs and better construction work.

The authors mentioned briefly the responsibility of inspector cooperation with other inspectors with whom he works. I think this point could be elabo-

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rated upon. Experience has shown that use of words is more effective than the so-called "club" in getting desired procedures followed. Inspector interests are similar and it behooves each inspector to cultivate a cooperative attitude on the job. Inspectors should not hesitate to ask questions of those with whom they work. Another worthwhile practice is to prepare for each assignment by reviewing the particular specifications for that job, in company with other inspectors, to prevent different interpretation of job requirements. We should be ever mindful of the fact that contractors will be more respectful of inspection if similar standards are required by all inspectors.

The authors have devoted the second section of their paper to "Relationship of Inspector to Contractor." This portion of their paper defines inspector responsibility regarding inspector-contractor relationships. The subject material presented by the authors in this section of their report, if practiced by the job inspector, will benefit both the inspector and contractor and reduce contract administration problems.

The important subject of inspector's cooperative and friendly attitude toward contractors was stressed by the authors. I would like to add a few personal observations on this subject to properly place the inspector's job in the important position it deserves.

Close contact with numerous construction contracts has provided me with an opportunity to observe inspector-contractor relationships on both smooth and troublesome contracts. Construction contracts completed in a workmanlike fashion generally have had good inspector-contractor relations, whereas on those contracts where construction difficulties occurred, we generally found differences had occurred between the inspector and contractor. Contractor's construction costs are increased when work delays occur as a result of replacing faulty work and future bid prices will reflect these increased costs. This places the inspector in a most important position. He can contribute toward lower construction costs in addition to maintaining approved work standards. How can he do this? I think the authors have provided the answers to this question and have supplied an excellent working guide for inspectors to follow on the job.

The expression that contractor's and inspector's objectives are essentially the same, to build a good job, is a true statement. It makes for smooth operation when both contractor and inspector work toward common objectives. Inspector conduct can influence contractor acceptance of inspection. Observations show considerable difference between inspectors. Some follow the practice of inspecting end results and take little personal interest in the contractor's methods, while other inspectors cooperate closely with contractors to not only inspect end results but contribute helpful suggestions and inspection tests during contractor operations.

There is a limit, however, to inspector cooperation. On one job the inspector cooperated by joining the contractor's concrete finishing crew, kneepads and all, working for both client and contractor for several days. The concrete

finish was good but the rest of the work, for which the inspector was responsible, did not receive proper inspection. This so-called inspector was soon replaced.

In the third section of their paper "Direction of Other Inspectors," the authors have presented in tabulated form, many inspection procedures for which supervisors are responsible.

The items numerated 1 to 7 are certainly recognized as important inspection details. To this list we might add additional items such as: (8) Stressing need for inspectors to be consistent in their day-to-day inspection requirements. Another item could be: (9) Review of contractors proposed schedule of operations in advance of actual construction. This should be done in a cooperative manner rather than critically, making known to the contractor the results of inspector's previous experiences and encouraging approved construction practice.

In the last paragraph the authors have mentioned the desirability of conducting inspector and supervisor meetings for the purpose of discussing job problems. The value of such meetings is well known to the writer who has been active in sponsoring meetings of this type.

On the Columbia Basin Project, our Bureau of Reclamation officials have, for years, approved the conducting of training sessions during winter slowdown periods. At these training sessions, the duties and responsibilities of the job inspector's position is reviewed. The need for making control tests on concrete and earthwork is stressed and the practice of controlling by "eyeball" methods discouraged. Illustrated lectures, discussions, and laboratory tests on concrete and soils are used to standardize inspection methods among our inspectors. Contractor forces and their material suppliers are encouraged to attend. They often take part and contribute their part toward the success of the training sessions.

I think the writer expresses the opinion of many American Concrete Institute members in encouraging that more articles of this type be included in future JOURNALS. The "Responsibility of an Inspector" provides field men with an excellent guide for reference use. Discussion objectives have been directed toward recognition of inspector responsibilities. Adoption of good practical inspector procedures will not only assure quality construction but result in reduced ultimate construction costs.

By J. W. KELLY*

Responsibilities of the construction inspector, so aptly given by Bloodgood and Tuthill in their charge to Bureau of Reclamation inspectors, are an important part of the necessary training and background for successful inspection. Everybody knows how to make good concrete, but too often the proper human relationships are not maintained and faulty concrete results. Greater recognition of the human factor is needed.

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Large organizations such as the Bureau of Reclamation, the Corps of Engineers, and state highway departments have well-organized training courses for their inspectors. Similar courses are needed for inspectors and potential inspectors of other and smaller organizations. Wherever public courses have been given, attendance and interest have been most gratifying. The Portland Cement Association has given many short courses for inspectors of the railroads and for concrete workers in general. A number of engineering colleges have held concrete conferences which include much valuable information for the inspector.

Experience in northern California is of interest. The University of California has given war training and extension courses and conferences as follows:

Courses	Number of courses	Hours per course	Enrollment
Concrete making and inspection	3	32	200
Design and inspection of concrete for highway structures	1	18	200
Inspection and control of concrete	15	16	400
Short course in concrete for the ready-mixed concrete industry	2	35	100
Question-and-answer session on practical concrete problems	7	3	1500

Such response to opportunities for study of concrete suggests that courses offered elsewhere would be well attended. They would contribute greatly to better understanding of concrete and to better relations between the inspector and the construction force.

By JOSEPH J. WADDELL*

Inspection of the highest quality is the objective of all competent engineers in the construction industry, and Messrs. Bloodgood and Tuthill have done a fine job of presenting a description of the principles, attitudes, and approaches suitable for an inspector.

A good inspector is a trained inspector, this training being implemented by lectures of the type presented by the authors. In addition, manuals and instruction memoranda should be provided covering the several aspects of the work. For instance, on the Northern Illinois Toll Highway there are several large casting yards producing prestressed bridge girders and piles for the project. At the time this phase of the work got under way, informal discussions were held with small groups of inspectors, and a mineographed pamphlet was distributed. This pamphlet is a brief outline of prestressed concrete operations and a manual of instruction covering inspection of these operations. It supplements the general inspection manual prepared by the consulting engineer.

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Another word of instruction that might be added for any inspector is: "Keep your supervisor informed." Too many times an inspector may feel that, having filled in the blanks on his report form, he has provided the necessary information. This, however, is usually not sufficient. The "remarks" part of the form is important too, for in this space the inspector can furnish information that cannot be inserted in spaces that usually call for numerical data.

It is the responsibility of the supervisor to know what is happening on the job, and while he obtains much of his information by observation of the work, he is nevertheless dependent upon the inspector for a large portion of his information. The inspector sees the work at the "grass roots" and is usually closer to detailed sources of information than anyone else in the engineer's organization. Not only on his written reports but by oral reports, he should apprise the supervisor of what is going on.

Discussion of a paper by M. W. Ferguson, G. L. Kalousek, and C. W. Smith:

Tests of a New Method for Evaluating Volume Changes of Concrete Masonry Units*

By S. B. HELMS and AUTHORS

By S. B. HELMS†

This new approach to the problem of developing a test procedure for volume changes of concrete block should attract attention of ACI members active in this field who realize the present need for a test suitable for specification purposes. This novel method should be tried by other investigators on block manufactured by other producers.

It may be assumed that the authors have obtained a large number of shrinkage test results—particularly of autoclaved cinder block—which are based on the rapid method as described in the 1953 report of ACI Committee 716. The authors indicate that the suitability of the rapid method for determining the shrinkage of low-pressure steam cured block needs further investigation, and they mention "the possible limitations of the rapid method as a test for shrinkage of low-pressure block."

It is probably true that any laboratory shrinkage test procedure will have some limitations if applied to a wide variety of samples containing different aggregates, different cementitious combinations, and cured by different procedures. The more widely any test is used the more likely there is a possibility of taking exception to it, and some persons will fear that the test indications will be false. Some authorities believe that block whether autoclaved or not are subject to a permanent reduction in volume when dried at temperatures above 212 F, as practiced in the rapid and new reverse rapid procedures; this effect could vary with the aggregate, the cementitious material, mode of curing, and the amount of CO_2 absorbed by the specimen during the test. There may prove to be a fundamental objection to reverse rapid drying because the specimen is oven dried before the measurement of re-expansion is taken.

Rehumidification of blocks was discussed in *Housing Research Paper* 34 (authors' Reference 2) and brief data of re-expansion of block were listed. These blocks, dried directly to 25 percent relative humidity, and in stages using intermediate levels of 85, 70, and 50 percent relative humidity, showed practically the same total shrinkage either way; after reaching equilibrium at 50 percent relative humidity, only a small residual shrinkage at 25 per-

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cent relative humidity was observed. Though not conclusive this latter finding indicates that accelerated drying to equilibrium at 73 F and 50 percent relative humidity, would be suitable for purposes of correlation with oven-drying shrinkage. Also, the authors' Fig. 4 and 5 should be compared with Fig. 26 to 31 of Reference 2, where controlled humidity conditions applied.

Incidentally, the British Standards Institution procedure for determination of moisture movement could be termed the "reverse British test." Tabular material in Easterly's ACI paper* pertains to the re-expansion of small specimens cut from high-pressure and low-pressure block, soaked in water 4 days, then dried to constant length. In comparison with the authors' footnote on p. 952, wherein two sets showed rapid test shrinkages of 0.036 and 0.044 percent respectively, Easterly's data show two sets derived from *same* high-pressure blocks with British test shrinkages of 0.023 and 0.030 percent respectively, and two sets from *same* low-pressure blocks with British test shrinkages of 0.056 and 0.043 percent, respectively.

Regarding the question of permanent length reduction—Easterly's Tables 2 and 3 show that nine specimens, including high-pressure or low-pressure cinder aggregate units, (all six specimens in Group C), re-expanded in 4-day soaking after British drying to the original 4-day soaked length. In contrast to this, the authors' comments on their Fig. 1 call attention to the case of the low-pressure block (Curve 1) which, after 48 hr drying at 225 F of the rapid test, re-expanded in 10 days soaking to a length 0.016 percent greater than the original 24-hr soaked length. This performance has never been observed in tests at our laboratory, but there have been cases where rapid method specimens regained the original soaked length as noted by Easterly. This apparent "growth" may simply prove that the original 24-hr soak was insufficient; on the other hand, it may be akin to the performance of the block, hard-dried at 400 F, illustrated by Curve 5 of the authors' Fig. 6.

Since the comparative shapes of re-expansion curves of high-pressure and low-pressure block are similar in Fig. 1 ("the rapid method test followed by a prolonged soaking period") and Fig. 3 ("reverse rapid test" results), there is some justification for making exploratory re-expansion test measurements on block previously tested by conventional shrinkage procedures.

For a comparative study, some high-pressure and low-pressure block were made using the same aggregate combinations and two types of aggregates. Our purpose was to compare drying shrinkage by rapid and British methods using whole block (the prescribed British procedure specifies 6-in. prisms of approximately 3 x 3-in. cross section). These tests were completed in February 1955 shortly after a report on tests by the reverse rapid method was presented at a meeting of ACI Committee 716. The claim that the reverse rapid technique would distinguish autoclaved block, when other methods might fail to do so, prompted us to study in detail the re-expansion properties of these particular test block. Our results will supplement the authors' paper,

*Easterly, H. W., Jr., "Correlation of Shrinkage and Curing in Concrete Masonry Units," ACI JOURNAL, Jan. 1952, *Proc.* V. 48, pp. 393-402.

since we obtained data for lightweight expanded slag block and heavy weight block. The authors report no re-expansion data for expanded slag and only one test for autoclaved block containing sand and gravel.

Fig. A contains separate curves for each of the eight test block under discussion. The curves are grouped separately according to the drying cycle used. The upper curves are for blocks previously dried to constant length under British conditions at 122 F and thereafter by "indirect rapid" drying to constant length in 216 F oven. The plotted re-expansions are based on the minimum oven-dry length. Similar behavior of lightweight block No. 2 and 5 is especially interesting because these particular autoclaved and low-pressure expanded slag units performed similarly in the regular shrinkage tests. For convenient scheduling these units had been soaked 72 hr prior to drying under British test conditions.

The lower group of curves in Fig. A represents blocks which had previously reached constant length in "direct rapid" drying in 216 F oven after 24 hr saturation. Again the plotted values for re-expansion of lightweight block No. 3 and 6 are of interest and the similarity agrees with the upper curves. The chief distinguishing characteristics of lightweight high-pressure block No. 5 and 6 were the lighter color and the fact that moisture content was no doubt lower when removed from autoclave.

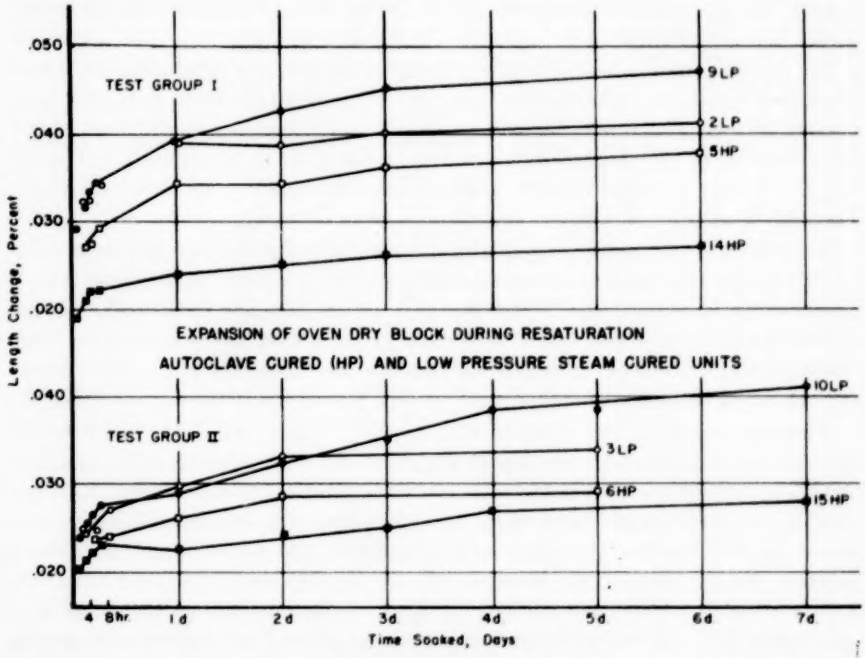

Fig. A—Measured re-expansion of lightweight blocks (numbered 2, 3, 5, and 6) and heavy blocks (numbered 9, 10, 14, and 15)

TABLE A—COMPARISON OF VOLUME CHANGE PROPERTIES OF TEST UNITS

Test Group I		Total shrinkage, percent		Total re-expansion, percent
Block No.	Description	British test	"Indirect" rapid	Reverse soaking
2	Low-pressure lightweight	0.026	0.045	0.041
5	Autoclaved lightweight	0.023	0.044	0.037
9	Low-pressure heavy	0.036	0.054	0.047
14	Autoclaved heavy	0.017	0.032	0.027

Test Group II		Total shrinkage, percent	Total re-expansion, percent
Block No.	Description	"Direct" rapid	Reverse soaking
3	Low-pressure lightweight	0.042	0.034
6	Autoclaved lightweight	0.036	0.029
10	Low-pressure heavy	0.046	0.041
15	Autoclaved heavy	0.032	0.028

The distinction between high-pressure and low-pressure heavy block was in line with the authors' findings and the curves for low-pressure block No. 9 and 10 showed a steeper rise, after 1-day soak, than autoclaved heavy block No. 14 and 15.

Brief mention of the care taken in making the measurements on these block is warranted. There were four gage lines on each block; two sets were dry set (driven plugs) and the other two sets had plugs grouted in. All gage lines were in excellent agreement during the shrinkage test phase but some dry set points in low-pressure block failed during the resaturation phase. The curves of Fig. A are for the reliable gage lines.

If the device were used of equalizing all curves on the basis of zero expansion at 24-hr soaking, the curves for lightweight block would be nearly superimposed in both test groups. Due to the number of gage lines used, the resaturation tests of these block were run in pairs and the schedule of later readings was varied according to laboratory working schedule. Full information on the shrinkage properties of the test block is given in Table A.

Without exception the order of magnitude of volume changes would result in the same arrangement of specimens whether listed in order of decreasing shrinkage or expansion. It is believed that considered objectively, allowing for the obvious circumstance that these low-pressure lightweight block had unusually low volume change properties, these tests indicate that the three volume change procedures have about the same degree of discrimination since the relationships are similar. This is the unique circumstance of this test series and the data are not reported to show low volume change for non-autoclaved lightweight block. The facts are well established that it is very difficult to consistently manufacture low-volume-change block except

by autoclave curing. However, when low-volume-change characteristics are found for low-pressure block by one procedure it appears that they should be found by the other procedures as well.

The work of the authors shows that attention should be directed to the comparatively short 24-hr soaking period used at start of conventional rapid tests, and it may be found that 48-hr soaking is more equitable in evaluating block of any type of curing. Also, as they have pointed out, the number of specimens in the oven affects the result if rapid tests are based on a fixed drying time interval of 48 hr. A definition of equilibrium length in rapid oven-drying tests appears desirable.

The phenomenon of "growth" in autoclaving shown in last column of Table 1 is not fully understood. Growth of this kind is not particularly desirable. Autoclaving block too soon after molding can result in non-uniform swelling or even cracking of the block. An appreciable expansion indicates unsoundness in autoclave tests of portland cement mortar but would not necessarily signify unsoundness when block are autoclaved.

In a study of soundness of selected samples of lightweight aggregate, using molded 2 x 2 x 11-in. lightweight concrete bars made from plastic 6-bag mixes of approximately 2-in. slump, we also obtained some incidental information on change in saturated length when autoclaved according to ASTM C 151. Each test mix contained the same Type I cement, and $\frac{3}{8}$-in. maximum aggregate was the principal variable. No substitutions of silica were involved:

Type of expanded lightweight aggregate:	Slag	Slate	Cinder	Clay	Slag	Slate	Shale
Percent expansion due to autoclaving:	0.026	0.059	0.151	0.050	0.023	0.092	0.123

These expansions are not necessarily characteristic of each general class of aggregate material; however, the varied expansion with one cementitious material indicates the nature of the aggregate has a considerable effect. While expanded slag might be expected to be characteristically low, possibly because of the method of manufacture, it may be assumed that cinders and expanded shales from many sources could show differences in performance in this "autoclave expansion" test of aggregate performance.

The data for a variety of concrete block given in Table 1 are interesting because rapid method shrinkage tests are shown to be of practical value in distinguishing between block of different shrinkage characteristics. There is evidence that comparison of the shrinkage of an open-market block with that of an autoclaved companion specimen block would be a reliable way to show whether a given masonry unit product was properly high-pressure cured. Except for the time taken to saturate and autoclave the companion block, this direct comparison could be accomplished in the short time it takes to complete a test by oven drying.

AUTHORS' CLOSURE

Mr. Helms has provided an interesting discussion of our paper, including the presentation of some useful data. The data on the lightweight slag blocks No. 2 and 5 in Fig. A, and also No. 3 and 6, are especially interesting, due to the similarity between their re-expansion curves. It would be perhaps desirable to have reautoclaved the autoclaved units to ascertain the degree of the original autoclaving process, if the autoclaved and low-pressure units were identical in other respects. Otherwise, the indicated similarity could possibly have been the result of incomplete autoclaving. Also, there is some evidence to indicate that repeated soaking and drying has a more pronounced effect upon low-pressure than upon autoclaved units. This could further partially explain the similarity in the aforementioned results. In tests run since publication of our paper, however, one author has encountered some difficulty in obtaining early stability for the re-expansion curves of autoclaved slag blocks by the reverse rapid method as indicated by Mr. Helms' results. Low-pressure companion specimens were not available for comparison, but reautoclaving reduced the reverse rapid method expansion for such units.

Discussion of a paper by Witold W. Zawilski:

Elastic Design of Prestressed Sections in Flexure by Charts or Tables*

By F. N. MENEFEE and AUTHOR

By F. N. MENEFEE†

Not since I first read Magnel's first and second editions of *Prestressed Concrete* have I been so intrigued with a treatise on prestressed concrete as I have with Mr. Zawilski's paper. Having taught the subject, my view of the article is probably slanted toward the pedagogical angle, with here and there an attempt to clarify some of the more obscure terms. That, however, does not detract from the value of the contribution Mr. Zawilski has made.

To begin with, I will admit that I had to study the article. This was due in part to the terseness with which it is written—no words are wasted. Also the terminology is somewhat different from that usually encountered. The approach is new and calls for a terminology adapted to it. For instance, dimensions and properties of the cross section are expressed in terms of the depth h of the stressed section. Most of them are tabulated on p. 962, and reference to the list of symbols, same page, along with Fig. 1, p. 968, illustrates how these symbols are used. For instance b_2 is the width of the top of the cross section, b_1 the bottom, but to begin with we don't know the quantitative value of b_2 or b_1; we merely know the designer has decided that they are to be 0.6 of the total depth of the cross section. The proportions shown in Fig. 1 are commonly used, based on experience. But to start with, the designer only knows the service (live) load which the beam must carry, from which M_s may be obtained; he knows the length L in ft, the value η, and the maximum allowable stress in the concrete. See Example 1, p. 967. Other values he computes making use of the dimensionless quantities a, y_2, y_1, α, h, and i. Once a is found, M_1 is easily found, using $ah^2 = A$.

At this point it may be pertinent to state that the designer wants the value of h in in. as shown in Fig. 1. Once he has that value all other dimensions are obtainable and he can then determine what w_1 and M_1 will be.

The small table at top of p. 962 could have had an H for "normal property" depth, and could have a dimensionless h multiplied by h (a dimension) $= H$, and in his list of symbols, eleventh line, the author could have written H, h = total depth of effective (stressed) section. Then using dimensionless h as 1.0 in the same way that he used $b_2 = 0.6$ in the basic equations developed

*ACI JOURNAL, Apr. 1957, *Proc.* V. 53, p. 961. Disc. 53-54 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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on pp. 964-967 and applied in Example 1, pp. 967-968, h (dimension) turns out to be 19.28 in., and 19.28 × 1 = 19.28 in. = H. Undoubtedly Mr. Zawilski saw all of this and either decided that a dimensionless h over a dimension h (table, top p. 962) giving H would be awkward or that the method he used was clear enough.

Brevity is important, but it would have taken but little space to have stated the units for such terms as A, M, etc., where two units are possible. This would have saved time for some readers which could be devoted to some of the rather obscure relationships. A is in sq in., h is in in., and moments as used in the equations are in in.-lb.

Having discovered a need for a coefficient in the use of the Stage 1 moment M_1 (dead load) see p. 963, he undoubtedly proceeded thus: for a simple beam uniformly loaded the maximum moment is $\frac{1}{8}\ w_1L^2$ = ft-lb. A is in sq in.; then $A/144 \times 150$ = the weight of the beam (w_1) per ft.

$$M_1 = \frac{w_1 L^2}{8} \text{ ft-lb, or } \frac{A \times 150 \times L^2 \times 12}{144 \times 8} \text{ in.-lb} = \frac{A \times 150 \times L^2}{8 \times 12}$$

Letting $150/(8 \times 12) = \nu = 1.5625$, a constant, we have $1.5625\ A\ L^2 = M_1$ in.-lb. Another term $y_2 = \alpha/(1 + \alpha)$ is extensively used but not derived. One derivation is as follows: $\alpha = y_2/y_1$ (p. 962); $y_1 + y_2 = 1$.

$$\frac{y^2}{\alpha} + y_2 = 1;\quad \frac{y^2}{\alpha} + y_2\frac{\alpha}{\alpha} = 1;\quad y_2\left(\frac{1+\alpha}{\alpha}\right) = 1 \text{ and } y_2 = \frac{\alpha}{1+\alpha}$$

$y_1 = 1/(1 + \alpha)$ can be similarly derived.

Signs of moments are just the opposite of those found in the conventional American text on mechanics of materials.

Eq. (1), p. 964, arises from the fact that F in Stage 1 becomes ηF in Stage 2, and that bending moment stresses produce forces which on a given cross section at a given time cancel out. That is, in a horizontal beam there are two resultant horizontal forces at a cross section due to bending moment, one tension and one compression but equal in amount. Hence, while there are stresses and forces due to M_1 and FE on the cross section at Stage 1, Σ horizontal forces = 0 for each of these moments. Fig. 1 shows an ideal distribution of stress at Stage 1, and if the section were assumed symmetrical about the neutral axis for sake of simplicity, α would be 1 and Eq. (3_1) could be written $F/A = (f_{21} + f_1)/2$ and since M_s in Stage 2 contributes no net force to the cross section, and F is reduced to ηF we would have $\eta F/A = (f_2 + f_{12})/2$ for Eq. (3_2), and f_2 would have to be smaller than f_1 (2130 psi in Example 1) which is contrary to the basic premise in the author's introduction. Hence, the neutral axis is shifted so that $\eta\ \alpha = 1$ or almost so; then f_2 (Fig. 1) = $\alpha\ \eta\ f_1$. From a practical standpoint there is a time interval between Stage 1 and Stage 2 where η will not have reached its usually chosen

low value of 0.85, in which case, since we can't change M_2 or α, f_2 will be slightly higher than f_1 during that interval.

So Eq. (1), (3_1), (3_2), and the one following are based on the fact that F on a cross section at Stage 1 $\times$ η = F at Stage 2.

Eq. (2_1) says that the internal resisting moment equals the external bending moments at Stage 1, and that the internal resisting moment is $(I/h)\,f_{1d}$. Had the author used I/y_1 $\times$ stress at extreme bottom fiber due to bending or I/y_2 $\times$ stress at extreme top fiber, instead of $(I/h)\,f_{1d}$, the reader would have agreed that the equation was correct as for moments, but that in arriving at the resultant top and bottom fiber stresses F/A must be considered. However, when confronted with $(I/h)\,f_{1d}$ for the first time and knowing that the stresses at top and bottom are a combination of direct F/A and our old f_{top} = My_2/I there is apt to be a halt in the study until the term f_{1d}/h is better understood. In $(If_{top})/y_2$, f_{top}/y_2 is a ratio or the slope of the line representing the variation in stress over the cross section and passing through zero at the neutral axis. The stresses due to moments FE and M_1 may be combined and the resultant top and bottom stresses found and the slope of the line representing the variation in stress will be

$$\frac{\text{stress at bottom}}{y_1} \quad \text{(a)}$$

$$\frac{\text{stress at top}}{y_2} \quad \text{(b)}$$

or

$$\frac{\text{stress at top} + \text{stress at bottom}}{y_1 + y_2 = h} \quad \text{(c)}$$

Mr. Zawilski doesn't want y_1 or y_2 in his formula; he wants h. He wants to make use of f_{21}, f_1, f_2, and f_{12}, symbols already in the record. But he wants a slope such as is given by (a) or (b) or (c) above. If the constantly varying stresses on the cross section due to bending moment resistance are represented by a sloping line and a constant stress F/A is added to each increment of y_2 and y_1, a new sloping line is created parallel to the one created by (c) above, but its slope may be expressed as f_{1d}/h as in (2_1) and as f_{2d}/h in (2_2). Under "Particular Equations" p. 965, Mr. Zawilski takes $f_{21} = 0$ and $f_{12} = 0$. Hence, one can make use of Fig. 1 to visualize the stress situation, and f_{1d} becomes f_1 in Eq. (5_1) because $f_{21} = 0$. The same reasoning applies in (5_2).

In Example 2, $t_1 = t_2 = b' = 0.15$, $L = 80$ ft, $M_s = 11{,}500{,}000$ in.-lb, and $\eta = 0.85$. α is taken as 0.8 and from these data and previously developed relationships $y_2 = 0.444$ and $y_1 = 0.556$. By balancing moments of cross-sectional areas about the middepth for a, using the first equation at top of p. 970, b_1, a, and i in terms of b_2 are found.

Note that on p. 967 Mr. Zawilski says "For I-beam when $\alpha = 0.80$, i = $0.05618\ b_2 + 0.00074$." This statement seems to admit of no exceptions and

yet in Example 2, $\alpha = 0.80$ and i turns out to be $0.04755\ b_2 + 0.00175$. This seeming contradiction is explained when it is known that $\alpha/c = 0.80/0.85$ gives an i of $0.05618\ b_2 + 0.00074$ in Table 3, while $\alpha/c = 0.80/0.75$ results in $i = 0.04755\ b_2 + 0.00175$ in Table 2.

In Example 2, b_2 was not chosen or assumed. It must follow from the other quantities assumed, as above given and shown on p. 969. The analytical solution for b_2 and other necessary dimensionless quantities, the outline of which is briefly shown on pp. 970 and 971, is lengthy and involved. However, Mr. Zawilski introduces equations (14_2) and (14_1) for terms ϕ and N respectively on p. 970 and explains how they may be derived on pp. 972 and 973, where they are referred to as Eq. (10_1) and (10_2). Again these derivations are tedious, particularly that for ϕ. Except for a sign in the denominator of Eq. (10_2) or (14_2) the writer has checked them. They are undoubtedly correct.

However, Mr. Zawilski comes out with an expression containing only the constant $\nu = 1.5625$, the known quantities, such as length of beam L, the service moment M_s and the term f_1, the value of which the designer chooses, all equated to an expression involving a chosen η and α along with dimensionless quantities such as those chosen in Examples 1 and 2. The designer does not know h or w_1. The analytical solution for them is long and tedious. However, as soon as b_2 is found they are easily determined along with any other terms dependent directly or indirectly upon b_2, as shown on pp. 970 and 971.

To avoid the analytical solution the author has developed tables and from them charts by which $1000\ \phi$ and $100\ N$ can be found at once for any chosen value of α. Reference to the terse statement in the middle of p. 971 under "Graphical solution of Eq. (14_2)" is of little help to the reader in his introductory study of the Zawilski method. But on p. 977, "Example 3, design by charts," there is a half page devoted to the graphical procedure (Fig. 3) which reduces the labor and time necessary for determining h to just a few minutes, a truly marvelous saving of time. I am still somewhat dazed at being able to solve for the cross-sectional dimensions of a beam without making an estimate of its weight per ft and solving for M_1. By Mr. Zawilski's method one can try out three or four values for b_2 and b_1 in the time formerly devoted to making an estimate as to cross sectional dimensions and trying them out to see whether or not they will provide adequate resisting moment to M_1 and M_s, with the stresses allowed.

Mr. Zawilski's treatise in its present form is not, in my opinion, well adapted to the needs of the beginner in prestressed reinforced concrete, but it was probably not intended to be. There are spots in it that will tax the ingenuity of the engineer fairly well versed in the usually accepted approach via $P/A \pm Pey/I$. But it serves well to introduce the more experienced designer in prestressed reinforced concrete to an exceptionally ingenious and quick method of solving practical problems.

Considering the amount of work that must have been devoted to providing symbols for quantities involved and to working out their relationships, and

to devising the charts and tables, Mr. Zawilski is to be highly commended for his contribution.

AUTHOR'S CLOSURE

The author wishes to thank Professor Menefee for his valuable clarification to the more obscure terms. Interesting criticism is raised regarding the assumption of limiting concrete stresses ($f_1 = 2130$) in Example 1. The author is in complete agreement with Professor Menefee. Generally it may happen in the case when $\alpha > 1$, $f_1 = 0$, $f_2 = 0$, and equation of stresses takes form $f_2 = \eta\,\alpha\,f_1$, that coefficient η will not drop below the value $\eta = 1/\alpha$ before service load reaches its allowable maximum, which causes temporary overstressing in concrete. When even during a short interval $\eta > 1/\alpha$, to be safe a limit should be imposed on stresses at the top fiber f_2, and the bottom fiber (Stage 1) should be stressed only to the value $f_1 = f_2/\eta\,\alpha$, in which η is maximum.

In Example 1, to prevent overstressing, the limiting concrete stress $f_2 = 2130$ instead $f_1 = 2130$ should be assumed, if during the action of maximum allowable service load the following inequality is satisfied:

$$\eta > \frac{1}{\alpha} = \frac{1}{1.105} = 0.905$$

In evaluating η at that time, the following main losses should be considered:

1. Losses due to slippage and deformation of anchorage
2. Losses due to elastic shortening
3. Losses due to partial creep of concrete and steel
4. Losses due to partial shrinkage of concrete

Assuming theoretically that total losses are zero, or $\eta = 1$ and $f_1 = 2130$, the following overstressing could be obtained at Stage 2:

$$f_2 = \alpha\,\eta\,f_1 = 1.105 \times 1 \times 2130 = 2350 > 2130$$

Discussion of a paper by B. G. Singh:

Effect of the Specific Surface of Aggregates on Consistency of Concrete*

By J. D. McINTOSH and B. W. SHACKLOCK, FRITZ SCHWANDA, H. N. WALSH, and AUTHOR

By J. D. McINTOSH and B. W. SHACKLOCK†

We are pleased that Dr. Singh has published a report of his work at the Building Research Station on the effect of specific surface of aggregate on the properties of fresh concrete; the earlier report by Newman and Teychenné (Reference 5) was concerned primarily with the effect on compressive strength. We were interested to note the relationships between the weight of water accumulating at the top of a 4-in. cube and the compacting factor and the water-cement ratio corrected for specific surface of aggregate. We would agree that with aggregate containing excessive fine particles, a higher proportion would be required for a given consistency than expected from the relationship between workability and specific surface, and this we attribute to the theoretical proportion being insufficient to fill the voids in the coarse aggregate. In fact, it appears from tests made on a variety of mixes using three types of aggregate that the compacting factor is more likely to be related directly to the specific surface of the aggregate used when the mixes are oversanded by normal standards.‡ Our experience confirms the fact that concrete made with gap-graded aggregates is more likely to segregate than the corresponding continuously graded mixes.

We feel, however, that the experimental evidence given in the paper is very scanty backing for Eq. (2). It seems that the first term in the denominator, involving P (the water-cement ratio of the paste of standard consistency for the cement under observation), is generally greater than either of the other two terms involving the aggregate-cement ratio, water-cement ratio, and specific surface of the aggregate. In view of the magnitude of the first term it is a pity that there is no indication of the number and range of values of P used in determining the value of the constant K_1 to justify the statement that all the factors considered are significant; in any case, we doubt whether there is any point in recording K_1 to three significant figures when values of P are probably only significant to two figures. We feel that this term is a matter of some importance because our experience indicates that K_1 may sometimes

*ACI JOURNAL, Apr. 1957, *Proc.* V. 53, p. 989. Disc. 53-55 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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‡Shacklock, B. W. and Walker W. R., "The Specific Surface of Concrete Aggregates and Its Relation to the Workability of Concrete," *Technical Report* TRA/241, Cement and Concrete Association, London, Jan. 1957, 15 pp.

be negative; an increase in the specific surface of the cement, which Dr. Singh states on p. 994 corresponds to a higher value of P, appears to produce an increase (and not a decrease) in the compacting factor of mixes with an aggregate-cement ratio of 9, although certainly for richer mixes the reverse appears to be true.

We believe, therefore, that the value of the paper would be greatly enhanced if Dr. Singh were to enlarge upon the experimental backing for his Eq. (2).

By FRITZ SCHWANDA*

Mr. Singh in a series of graphs presents the relationship between consistency of a concrete mix and specific surface of aggregate for constant W/C ratios.

The right ends of the curves show how we can improve the workability of a mix consisting of an aggregate with a great specific surface by reducing the proportion of fines. If the workability of the original mix was sufficient we can reduce the water added and increase the strength instead. If both workability and strength had been sufficient, we could save cement by eliminating the excess content of the finer size groups. The point where the optimum balance between saving of cement and waste of aggregate is reached is easily determined. All that can be deduced from the right branches of the curves.

Still more interesting are the left ends of the graphs, where the linear relationship between consistency and specific surface comes to an abrupt stop. That is of consequence if a mix is to be proportioned with aggregate deficient in the finer sizes. In this case part of the coarser constituents must be wasted or crushed, or fine material must be added. The graph shows that there is a minimum point of specific surface which at any rate must be reached, but which should not be passed, as any excess of fines added not only causes extra costs but also reduces the workability (and also reduces the strength or augments the cement demand). The accurate position of that dividing point is therefore of interest.

If we consider a mix similar to that which the author called No. 1 (Table 1, p. 992) consisting only of the two size groups No. 100–No. 52 and $\frac{3}{8}$ in.–$\frac{3}{4}$ in., the limit between workable and unworkable mixes is reached when there are just enough fine particles to fill the interstices between the coarse. If more fine material is added, the specific surface increases and the workability decreases, as more cement paste is consumed to cover every single aggregate particle, and the total void space which is to be filled is increased too; we are on the right branch of the curve, which the author has drawn as a solid line. If on the other hand, we reduce the content of fines below that at the dividing point, the great interstices between the large particles are no longer completely filled with small ones, larger voids form, the chains of cement particles which bridge them grow longer and more likely to be broken at some point; the fines tend to accumulate toward the bottom, pressing the cement paste upward. The mix begins to segregate, becomes more and more

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unworkable, and yields hardened concrete of poor quality. We are on the left branch of the curve which the author has drawn in a dashed line.

Conditions are essentially the same if the grading is composed not only of two size groups but of more, but matters are complicated by particle interference. Summing up, it can be said that only such gradings yield workable mixes where the coarse particles are safely embedded between the fines and where no interstices occur which are larger than those in an aggregate passing No. 50 or 100 sieve. The better the aggregate grading, the better the particle shape, the smoother the particle surface, the smaller the W/C ratio, the farther to the left of the graph (presenting coarser mixes with smaller specific surfaces) is situated the limit between workable and unworkable gradings. Also the cement, especially its fineness of grind, is of influence.

By H. N. WALSH*

The use of the word "discredited" in relation to Edwards' surface area method in the first paragraph is unfortunate and historically inaccurate. One might just as well say that Abrams' fineness modulus method was discredited when Williams and Davis[1] and later Talbot and Richart[2] published results showing no relation between fineness modulus and strength except in one series of tests by the latter pair. But this would be as untrue as the author's statement. On the positive side, in 1920 Young[3] published an account of the successful practical use of W/C to control strength and surface area to control workability in designing concrete for the Hydro-Electric Power Commission of Ontario.

In the controversy about these methods the value of Abrams' rediscovery in America of a particular case of Feret's more general results published in 1892 and 1897[4] together with the ease of measuring fineness modulus gave the use of the latter far greater popularity. Neither method satisfied many engineers. In Europe it was found much more satisfactory to base design on the sieve analysis itself rather than on a derivative or function of it. This point of view has grown throughout the world and methods based on it are widely used.

The author's own results appear to be a denial of the so-called discrediting of Edwards' work insofar as his diagrams of Fig. 1 and his expression of compacting factor as a function of specific surface are concerned.

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3. Young, R. B., "Mixing Concrete by Surface Area of Aggregates," *Engineering News-Record*, 1920, Part 1, p. 33.

*Consulting Engineer, Cork, Ireland.

4. Feret on mortar voids and strength, *Annales des Ponts et Chaussees* (Paris), July 1892, and *Bulletin de la Societe d'Encouragement Nationale*, Dec. 1897.

AUTHOR'S CLOSURE

I am pleased to learn that the specific surface idea is being pursued at the Cement and Concrete Association. The contribution made by Shacklock and Walker and cited by McIntosh and Shacklock is very timely. I believe that investigation at the Building Research Station is continuing.

It is quite true that as the aggregate grading gets harsh the CF-specific surface relationship breaks down as shown in Fig. 1. However, the limit at which the relationship starts to break down is not independent of the aggregate-cement ratio. Of course the breakdown of, or conformity with, the relationship cannot be taken as an indication of suitability of a mix. The suitability of a mix must necessarily include other factors. Also we should not lose sight of the fact that the compacting-factor method does not give an absolute measure of consistency, but only an indication of it.

Admittedly my experiments were restricted to examining the effect of the specific surface of aggregate, and the effect of the cement was included since three cements were used with P values 0.265, 0.275 and 0.280, though the cement with $P = 0.280$ was used to much greater extent. Results were analyzed statistically and Eq. (2) obtained.

P was included because I felt, probably without sufficient justification, that it is of importance in influencing consistency. Regarding the other variables, it should be obvious from Fig. 3, and the description given, that water-cement, aggregate-cement ratio, and specific surface varied over a wide range and Eq. (2) fits the result best, though it is unlikely to be of application if the P value of the cement falls outside the limits of 27 and 28 percent. Thus I should agree with Messrs. McIntosh and Shacklock that there is little point in using K to three significant figures, if Eq. (2) is used in a more general way.

The effect of cement type on consistency is of great interest and is likely to be a challenge for a long time. Cement, unlike aggreagte, is chemically active, and its rate of chemical reaction and the amount of water associated with it in a mix will undoubtedly influence the properties of the cement matrix. Conceivably two cements having the same P value, might not necessarily influence concrete mixes at comparable water-cement ratios in the same way. We are probably approaching the point when only a better understanding of cement matrix will lead to further progress in the understanding of the consistency of concrete.

Dr. Schwanda's comments considering implications of the paper in relation to concrete mix proportioning are encouraging. Though the graphs show clearly that consistency becomes stiffer as the specific surface of the aggregate is increased, the relationship between consistency and a suitable aggregate specific surface for a given mix necessarily depends on many factors not considered in the present paper. Even the breakdown of the relationship between

CF and specific surface at constant water-cement ratio should not be taken to mean that the mixes at such lower specific surfaces are unusable.

There must be a specific surface value for given fine and coarse aggregate gradings and conditions of placement, in accordance with the theoretical arguments advanced by Dr. Schwanda, though further research is required to deduce it to finer limits. Published results show that as a sand becomes finer, less of it can be used with advantage, although a higher specific surface must be used to overcome what is arbitrarily defined as harshness.

No claims are or should be made that the specific surface concept explains the effect of aggregate on consistency *fully*. Apart from further research required on other aggregate factors affecting consistency, much research is still required to establish gradings for aggregates of given physical properties, that with minimum specific surface, water and cement content will fully compact for given conditions of placement and required strength.

Professor Walsh objects to saying that the idea of surface area of Edwards was discredited. Now I do consider many of Young's (and Edwards') arguments in favour of the specific surface of aggregates as "still valid." I will further state that these papers, referred to by Professor Walsh, were the main source of my inspiration.

Results from research published a few years after the Edwards and Young papers referred to, and following Duff Abrams' paper on proportioning by the water-cement ratio and fineness modulus method, were conflicting and the discussions that followed were, to a large extent, prejudiced and irrelevant. And it is beyond argument that Abrams' ideas were adopted and the surface area method eclipsed.

I know of no studies between 1923 and the present on the surface area of aggregates. If the idea were not devoid of credit and were considered to hold possibilities, might we not expect a single publication over more than three decades of unflagging concrete research?

The papers by Williams and Davis and Richart and Talbot may have shown no relation exists between the fineness modulus and strength, but the point made by Professor Walsh would have had greater force if it were shown that no relationship existed between fineness modulus and *consistency* which is more relevant to the present discussion.

I am sure Professor Walsh does not wish to criticise me for pursuing an idea that was deprived of credit or "discredited" for more than three decades. Under the circumstances, perhaps he will forgive me for saying that the idea of aggregate surface area was discredited. I even hope I have succeeded in bringing to this idea some small measure of credit.

It can be shown that the limits of fine and coarse aggregate gradings that could be used with the method advocated by Professor Walsh could also be used with almost the same reasonable degree of confidence with Abrams' method. Abrams' method would also be of use when other fine and coarse aggregate gradings are used. There are obviously practical reasons why Americans have continued to use Abrams' fineness modulus.

Discussion of a paper by Walter E. Riley:

Analysis of Continuous Arches on Flexible Piers*

By GUNHARD-AESTIUS ORAVAS and AUTHOR

By GUNHARD-AESTIUS ORAVAS†

This paper has overlooked all significant contributions in the analysis of continuous arches on elastic piers,[1,3,4] including a textbook,[2] published in the last 25 years.

The method of solution demonstrated in the paper has been well known for more than a decade. The content of this paper is an almost identical reproduction of a recent paper by Michalos and Girton.[3]

It is unfortunate that the author apparently did not take the trouble to survey at least the readily available domestic literature for existing contributions on the subject. The inevitable result is that a lot of effort goes into a treatise that at its best is only a duplication of an already existing paper, which on the other hand receives no credit by the author for being the original contribution.

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3. Michalos, J., and Girton, D. D., "Continuous Arches on Elastic Piers," *Proceedings*, ASCE, V. 81, Nov. 1955, Paper No. 827, pp. 827-1—827-22.
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AUTHOR'S CLOSURE

In answer to Mr. Oravas, the author would like to point out that he has been using the method of analysis given in the paper for over 10 years for various structures of concrete and steel. The paper was originally written in February, 1950 for the design staff of the author's office, and was never released for publication prior to its ACI appearance. This 1950 date may be verified by a copy of the original unpublished paper on file in the University of Arizona Engineering Library.

As to the bibliography given by Mr. Oravas, the author is quite familiar with these publications as well as others not given. Except for Reference 3

*ACI JOURNAL, Apr. 1957, *Proc.* V. 53, p. 999. Disc. 53-56 is a part of the copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.
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the methods of solution are awkward and not an extension of the moment distribution method. For instance, Hrennikoff does not use carry-over factors and redistribution, but requires two equations in two unknowns to distribute forces at each joint.

Regarding Reference 3, I agree that this paper by Michalos and Girton uses the same approach with the possible exception of the determination of thrusts; however, their paper was published by ASCE 5 years after the author's original paper was written, so it would appear that originality cannot be claimed by them. It was not the author's purpose to seek credit for being the original contributor to this subject.

As stated in my letter transmitting the manuscript to the Institute, "No new theories are proposed. It is really the utilization of available knowledge in a manner which makes it possible for the practicing engineer to analyze multiple arches and not leave it to the theoretically minded group as in the past . . . Please note the broad range of arch types given in the tables. This makes possible its use in bridge design, as bridge arches usually lie above a true parabola."

If the paper aids the practicing designing engineer and encourages him in the use of structures of this type, the writer will be more than rewarded for the effort expended in its preparation.

Discussion of a report by ACI Committee 605:

Hot Weather Concreting Problems*

By CELSO A. CARBONELL and WILL M. HEISER

By CELSO A. CARBONELL†

I thank the committee for having tackled one of our main problems. The introduction to this article says, "That hot weather creates special problems of concreting there can be no doubt. This seems to have come as a surprise to some. That there should be cold weather difficulties—Yes! But hot weather, why?" I do not know why hot weather problems in concrete should be a surprise—in our climate it is the main problem which we face daily.

The article is well presented, simply and understandably. Not only articles but complete books could be written on this subject. The introduction further says, "We have higher early-strength cement which means finer grinding and modifications in compound composition contributing to rapid gain in strength and rapid generation of heat." This should not be a problem if the type of cement is selected taking in consideration the work in which it will be applied. In tropical countries this is of utmost importance, for many times the cement used is not the one recommended for the work involved.

MINIMIZING HOT WEATHER PROBLEMS

Materials

"Problems of excess water demand and strength reduction may also be alleviated in some cases by the judicious use of admixtures." If the committee says by the judicious of admixtures, it should state that in most cases, not in some cases, the problem will be alleviated. Admixtures are tools that, if used properly, give the engineer complete control of the situation.

I agree that "the most direct approach to keeping concrete temperature down is by controlling the temperature of its ingredients." However, nothing has been said about the cement factor. Cement generates heat, and if we can keep the cement factor at a minimum, we therefore minimize the generated heat. Consequently, I think that it is important in hot weather to maintain cement content at a minimum compatible with the type of concrete.

Production and delivery

A study should be made of delivery and mixing time, taking in consideration the distance from plant to job and the type of concrete transported. Proper use of an admixture will help to solve these problems.

*ACI Journal, May 1957, *Proc.* V. 53, p. 1025. Disc. 53-57 is a part of the copyrighted Journal of the American Concrete Institute, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

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Placement and protection

It is important to check completely the forms and the site. Many times plastic cracking is due to errors in the forms. One of the best ways to control plastic cracking is by revibration. This can be facilitated by the judicious use of retarders.

I consider that curing is the most important matter in hot weather concreting operations. The curing should start as soon as possible, and it should be continued without interruption for at least 8 days. Many times this is not done, and difficulties of all kinds arise.

Concrete testing

In many cases there are inclinations to disregard the importance of testing the concrete. In all kinds of concrete operations, the preparation, handling, and curing of the cylinders should be conducted in strict accordance with standard procedure. Cylinders should not be moved from their position during the first 24 hr, and then should be moved only to a carefully selected place. Perhaps the best way will be to have two sets of cylinders, one standard cured and the other job cured. Sufficient cylinders should be made to have a complete picture of the concrete; test results should be properly analyzed.

By WILL M. HEISER*

The progress report of Committee 605 is a valuable summary of the problems and remedies possible in connection with maintaining good quality in concrete placed in hot weather. This general problem has been of much concern in Pakistan where so much of the year involves hot weather concreting. Our experience here has been as consultants on the design and construction of the powerhouse and miscellaneous structures for a 13,000 kw low-head hydroelectric project.

From April to July the daily temperatures range from 80 F at night to 115 F during the day, and the humidity is very low during this entire period. From the latter part of July to September, the daytime temperature remains around 100 F, but the heavy monsoon rains during this period keep the relative humidity high. In concreting most difficulties have occurred during June and July when the temperature rises to 115 F every day for several weeks straight. During this period the minimum temperature at night seldom falls below 90 F.

MIXING, PLACING, AND CURING

Most concrete is placed from 5 a.m. to 10 a.m., primarily for the convenience of the labor crew since it is almost physically impossible to continue working during midday heat. Concrete is mixed using standard ½ cu yd batch mixers powered by electric motors, emptied into small rail cars, pushed manually about 100 yd, dumped on a flat steel sheet, shoveled into ½ cu ft

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metal baskets, carried by head labor to the final location where the concrete is dumped into chutes. The chutes are supported on a timber grill resting on pipe supports 5 to 10 ft above the recently cast surface. The timber grill provides access over various portions of the block being placed and also partially shades the concrete. As soon as part of the concrete is placed in a particular area, burlap is laid over the timber grill and kept damp. As soon as possible, water curing is started over the entire area. The surface is kept wet for at least 7 days and where level surface permits, the water is allowed to stand on the surface by ponding within small sand dikes.

In the massive substructure, lifts of concrete are about 5 ft thick and are allowed to stand 5 to 7 days before succeeding lifts are placed. The maximum horizontal dimension of such blocks does not exceed 50 ft. Cracks due to plastic shrinkage occur because of rapid surface drying after concrete is placed, when the surface is not sufficiently shaded and water curing has not been started soon enough. It is recommended that concrete should not be placed after the temperature in the sun has reached 105 F. It is often difficult to schedule work such that all concrete is placed during the cooler part of the day. It is, therefore, also recommended that the area be kept shaded as much as possible and that the surface never be allowed to dry for even a short time. Wet burlap even on freshly placed concrete will help in reducing shrinkage cracking due to rapid drying.

Keeping the concrete placing temperature low by pre-cooling aggregate and water is very important. On Chichoki Hydel Project the mixing water is pumped directly from tubewells which are actually used to lower the water table to enable construction work of the foundation to proceed in a dry condition some 40 ft below ground water level. These wells extend 100 ft below the ground and the water temperature is about 69 F.

On a project where much of the work is done manually and modern construction plant is at a minimum, it is particularly important that concrete should be moved from mixer to final position as quickly as possible with minimum handling. This avoids exposure to heat of the sun and high air temperature as well as the heating effect of manual handling, and also reduces the likelihood of aggregate separation during handling.

RECOMMENDATIONS

The following recommendations already stated in the committee report have also been found in our experience to be helpful in improving the quality of hot weather concrete.

1. Use cool mixing water.
2. Keep aggregate temperature as low as economically feasible.
3. Reduce the length of mixing time.
4. Place concrete as soon as possible after mixing with a minimum of handling.
5. Keep the surface shaded during placing.
6. Start water curing as soon as possible and keep thoroughly damp for several days. The surface should be kept wet and not allowed to dry for as long as 20 to 30 days when left in an exposed position.

Discussion of a paper by Martin Schulz and Mauricio Chedraui:

Tables for Circularly Curved Horizontal Beams with Symmetrical Uniform Loads*

By TUNG AU, KARL KRAUSCHE, and AUTHORS

By TUNG AU†

The solution of circularly curved horizontal beams fixed at supports with symmetrical uniform loads has previously been treated by Oesterblom[1] and others. Many problems of this type encountered in actual practice can be solved by the assumption of uniform loads with fair approximations. A recent example is its application to the solution of helicoidal staircases of reinforced concrete.[2] The present paper offers tables intended to simplify and expedite the calculation of circularly curved horizontal beams. However, the usefulness of these tables seems rather limited for several reasons.

Following the notation of the paper, let the width of the cross section be denoted by a and the depth by $b = ka$. Then, on basis of St. Venant's formula for torsional rigidity of the section, it is shown in the paper after Eq. (17) that

$$\mu = 0.65\left(1 + \frac{b^2}{a^2}\right) \qquad \text{(a)}$$

This expression can be calculated in the most straightforward manner without the aid of Table 1, and the ratio need not be limited to the range between 1 and 2 as indicated in the table. It must be noted, however, that St. Venant's formula may not be acceptable for rectangular section of large b/a ratio since it is approximated by the torsional rigidity of an equivalent elliptical cross section having the same cross-sectional area and the same polar moment of inertia. For this reason, it may be desirable to tabulate and compare the values of μ based on the torsional rigidity obtained by more accurate methods. One such method is membrane analogy by which the differential equation of the problem can be formulated. The solution of the equation by infinite series converges rapidly and leads to the following:[3]

$$J' = \frac{Mt}{G\theta} = k_1 a^3 b \qquad \text{(b)}$$

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where k_1 is a numerical factor depending on the ratio b/a. As a result, the formula for μ becomes:

$$\mu = \frac{0.196}{k_1} \frac{b^2}{a^2} \quad \text{.....(c)}$$

A comparison of the values of μ by Eq. (a) and (c) is shown in Table A. It is obvious that for ordinary proportions of beams encountered in actual practice Eq. (a) is sufficiently accurate for design purpose. If further refinement is necessary, the tabulated values should be based on Eq. (c), which checks with values given by Bergman.[2]

TABLE A

b/a	k_1 from Reference 3	μ by Eq. (a)	μ by Eq. (c)
1.0	0.1406	1.30	1.39
1.2	0.166	1.59	1.70
1.5	0.196	2.11	2.25
2.0	0.229	3.25	3.42
2.5	0.249	4.71	4.92
3.0	0.263	6.50	6.70
4.0	0.281	11.05	11.15
5.0	0.291	16.90	16.85
10.0	0.312	65.60	62.80

Table 2 of the paper gives values of N for angle ϕ_o up to 90 deg. At increments of ϕ_o, the magnitudes of N given for three different values of μ in the table show little difference. However, for ϕ_o greater than 90 deg, the variation of N for different values of μ becomes very significant. Let $N = U - 1$. Then Eq. (18) of the paper becomes:

$$X = q\, r^2\, (U - 1) \quad \text{.....(d)}$$

A graph by Bergman[2] showing the relation of U and ϕ_o for various values of μ is most revealing. It seems that such graph has a wider latitude and appears more suitable for calculating X in Eq. (d).

In the paper by Oesterblom,[1] graphs have been provided for the solution of Mf_m, Mf_a, Mt_i, and Mt_a for angle ϕ_o from 0 to 90 deg. It also contains graphs showing the location of critical points for zero bending and zero torsion as well as a thorough interpretation of the problem. These graphs include cases for ratio b/a = 1, 2, and 4; they serve the same purpose as Table 3 in this paper.

Horizontally curved beams with different shapes and with various loadings have received wide attention in recent years. Many interesting cases of circular arc bow girders under distributed and concentrated loads are discussed by Pippard.[4] Solutions of uniformly loaded bow girders for which the center line is a cycloid, a catenary, or a parabola have been obtained by Volterra.[5] Exact and approximate formulas are given by Abbassi[6] for symmetrical bow girders of any shape carrying uniform loads or two equal concentrated loads placed symmetrically with respect to the midpoint of the girder. Simplifications in calculation procedures are still needed in applying the more complicated solutions to actual design.

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By KARL KRAUSCHE*

The authors have investigated a rarely used supporting structure for a particular case of loading and have given the results so that in exactly the same cases a quick and convenient calculation is possible.

At first the statically indeterminate quantities had to be found. But since in case of a general handling of forces six such quantities appear, it would have been right to prove that here is the only one to consider.

The work gives rise to a general consideration: Its application is limited and the results will seldom be used. An engineer who has to carry out a certain assignment would have to: (1) remember to have read such a test; (2) remember where to find it; (3) try to find it; and (4) examine whether he can use it and how.

A specialization for rare cases makes the advantage of such efforts questionable, but if one expanded the range, the usefulnes would increase considerably. For instance, it would have been more practical—instead of assuming a full loading of the supporting structure—to take a single loading at any given point to bring the equations for the three statically indeterminate quantities into convenient form and, to make it more practical for calculation, to give the general formula for these quantities. The calculation of the prepared formulas often means only greater effort and increases the danger of mathematical errors. Whether or not one should make tables for some of the sizes would have to be considered. Then when a certain case of loading occurred, the result would be easy to find, also for divided loads, either by simple integration or with the help of chosen equivalent single loads.

And another general consideration, would it not be very rewarding for a large engineering society to put today's countless formulas and tables together and work out a standard manual for the most useful materials? This would certainly save much time.

*Engineer, Lübeck, West Germany.

AUTHORS' CLOSURE

The authors thank the writers who have discussed their article; they feel that the contributions have added materially to the value of the paper.

With reference to Professor Au's discussion there is the following to be said: For values $\phi_o = 90$ deg and values $b/a = 10$ the method followed by the authors is sufficiently exact for practical purpose. Beams with $\phi_o > 90$ deg and $b/a > 10$ seldom occur in actual practice. In case of a narrow rectangular cross section the membrane analogy gives more exact solutions of the torsional problem.

The suggestion made by Dr. Krausche about gathering all available formulas and tables concerning circular beams in form of a manual would be very helpful for the practicing engineer.

Discussion of a paper by E. Van Walsum:

An Idea for Long-Span Prestressed Concrete Bridges*

By UKU MÜLLERSDORF, M. R. ROŠ, LUIS SAENZ and IGNACIO MARTIN, and AUTHOR

By UKU MÜLLERSDORF†

I have noted the views of Mr. Van Walsum with the greatest interest, and I must admit that the idea of using hyperboloidal shape even for girders is attractive. This form has already been employed for water reservoirs in conjunction with prestressing (*Travaux*, 1948, p. 210).

Of course it is impossible, without going through the figures of a specific project, immediately to determine wherein the strong and weak points of the system may lie. I suppose however that it may be applicable to continuous bridges with 150 to 300-ft spans. With smaller spans I think that in general it would be difficult to compete both with ordinary reinforced concrete and with prestressed girder bridges with less exacting formwork. Moreover, it would also be difficult to design the section in the middle of the span, as the structural depths are probably too low. With spans exceeding 300 ft the scaffolding constitutes a very great financial burden and in my opinion in that range it would be difficult to compete with the construction method introduced by Dr. Finsterwalder. I may mention that we have in our office designed a bridge with a span of 850 ft to be built by this latter method.

In designing a bridge with hyperboloidal shape it should be observed that the section has a comparatively low section modulus on the underside, and this may give rise to trouble with supports having negative moment. Local reinforcement may remedy this. Transversely the section should be dimensioned to provide for moments due to shearing forces—analogous with shell structures.

As I understand it, the question of the friction is most important in determining the shape of the girders. Of course, it is good to have the cables straight, but it should not be overlooked that friction exists even with straight cables. Experience has shown that cables of greater length than about 250 ft should preferably not be used unless losses are compensated by the arrangement of prestressing points between the anchoring points. In statically indeterminate systems the efficiency with straight cables is usually less than with curved cables. It is easy to realize this if one considers a built-in girder. This difference is greatest in a nonhaunched girder, but appears to be noticeable in haunched girders as well.

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A scaffolding is always a disquieting feature and with prestressed bridges in particular, caution should be observed, as prior to the prestressing the structure is practically speaking without reinforcement and settling may lead to damage which is extremely difficult to repair. From this point of view the method of Dr. Finsterwalder is very sure.

I do not agree with the statement that this type of structure represents a sacrifice of economy in the placement and stressing of the steel. In each casting stage 16 to 20 steel bars are stressed and usually there is no static reason for stressing all or some of them several times. If at times a steel bar is prestressed in more than one point this is because it is desired to avoid excessive bar lengths (prestressing and grouting lengths), preferably not more than 150 ft. Usually the prestressing elements go through two sections each 10 ft long and are spliced every 20 ft, not every 10 ft.

In conclusion: (1) It is unnatural to haunch the beam over a momentless support. Increase of section over the end support as in Fig. 3 increases the shearing forces. (2) The execution with prefabricated deck plates and prestressing in the transverse direction seems rather complicated. Owing to the variable width of the box section all these elements must vary throughout the span.

By M. R. ROŠ*

Mr. Van Walsum is to be congratulated for having developed a new solution, departing from the traditional beam, slab, or rectangular box shapes. It seems that due to the distribution of the moments of inertia the proposed design is particularly suitable for continuous bridges, and where it is preferred to have the bridge supported by single columns only, because the torsional rigidity of the section may then be used to advantage. Another interesting possibility is the application to the cantilevering-out method of construction avoiding any forms supported from the ground.

It would be advisable to complete the proposition by a stress analysis, including as well the effects of the induced moments. Unless this is done it is difficult to evaluate the possible merits of Mr. Van Walsum's design, but I feel myself quite tempted to consider it as an alternative in one of my next projects in spite of the fact that forms and concreting of a hyppar bridge will be more complicated than for the traditional types.

By LUIS SAENZ and IGNACIO MARTIN†

The design proposed by the author to solve long-span prestressed concrete bridges is undoubtedly ingenious. The hyperboloid shape of the box girder would be striking in any bridge, but it may be doubtful that the hyppar bridge could meet successfully the six basic requirements mentioned by the author to obtain a good solution for a continuous prestressed bridge.

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†Partners, Saenz-Cancio-Martin, Ingenieros, Havana, Cuba.

—Comparing the design of a box girder type, similar to the Cuyaguateje Bridge, with a hyppar design for the same conditions, the following disadvantages would be found:

1. For the same depth, the rigidity of the parabolic or circular section is considerably less than the rigidity of a box-type girder, where interior vertical walls can be added to increase the moment of inertia of the section.

A hyppar bridge with the same depth as the Cuyaguateje River Bridge, would be approximately four times less rigid than the actual box girder design. In the former bridge the dead and live load moments are not fully compensated by the prestressing forces for the sake of economy in the design. A hyppar bridge with the same depth and span and under the same conditions would have important deflections, due to its lack of rigidity.

In order to increase the rigidity of a hyppar bridge it is necessary to increase its depth, but if the depth is increased the bridge would loose its slender appearance, and the deck would have to be raised accordingly.

In a box girder bridge the strands, in the sections of maximum moments, take full advantage of the lever arm of the prestressing force, while the strands in the hyppar bridge do not have the maximum lever arm allowed by the depth of the section, due to the position of the strands. However, the centroid of the midspan section is higher in a hyppar bridge than in a box girder bridge. The effect of the position of the strands and of the position of the centroid seem to compensate each other, giving about the same prestressing moment in both types of bridges at midspan, but at the support section, the strands are lower in the hyppar bridge and the lever arm is smaller than in a box girder, making it necessary to increase the number of deck strands; thus increasing the cost of the hyppar bridge.

2. The deck overhang is variable from a minimum at the supports to a maximum at the center of the span. The steel reinforcement of the overhang would be variable too, thus complicating its placement, even if the deck is precast. The variable deck overhang fails to meet the fourth basic requirement mentioned by the author, "easy placement of steel."

In the case of the Cuyaguateje River Bridge, a hyppar bridge with circular section and a 6-ft depth at midspan, would have an overhang more than 8 ft long, which is not an economical design for a deck slab supporting AASHO H20 loading.

3. The deck has only two supports in the case of the hyppar bridge. For a depth of 12 ft at the support and a circular section, the slab span would be 24 ft, which cannot be regarded as an economical span for AASHO H20 loading.

4. Placement of steel in a curved form is more difficult than in a vertical wall, which again fails to meet the fourth basic requirement stated by the author.

5. There are no friction losses in a hyppar bridge, which means a saving of about 10 percent in the prestressing force, but it requires a very careful

placement of strands inside of a curved form at different elevations in each section.

6. It is necessary to tension each strand from one end only in the hyppar bridge, but it is necessary to tension strands from both ends of the bridge. The cost of tensioning the strands is small with a thread and nut anchoring system. In the hyppar bridge, strands are tensioned from the outside of the bridge while in a box type bridge the strands are tensioned from an inside chamber. Even if 50 percent of the tensioning operations are saved in the hyppar bridge, the outside tensioning at different elevations in each cable is more costly than prestressing groups of strands at the same elevations in a tensioning chamber.

7. Shear stresses are reduced in prestressed structures. In the case of the box type girder, the influence of shear stresses in the design of the bridge is very small. The transverse cable and its tensioning is an added cost for the hyppar bridge design, the practical result of which should be justified.

8. A good solution to the formwork of the hyppar bridge is given by the author, but the hyppar formwork is more complicated than the box girder formwork, which can be done by common craftsmanship. The formwork of a box girder bridge is more in accordance with the first basic requirement given by the author, "simple formwork."

The writers make the preceding comments on the comparison between the box girder and hyppar bridge types with the idea of suggesting possible improvements of what they consider an ingenious way of spanning wide spaces with concrete structures.

AUTHOR'S CLOSURE

I thank Mr. Roš for his appreciative commentary and I sincerely hope that we will have an opportunity to work together in the future on a hyppar project.

I am most grateful to Messrs. Saenz, Martin, and Müllersdorf for their critical observations regarding the idea of applying the hyppar shapes to bridge construction.

They compare the hyppar type bridge with two proven methods. In several points the observations made and the conclusions drawn therefrom are correct. I agree, for instance, with Messrs. Saenz and Martin that a hyppar bridge generally would have to be deeper at midspan than a bridge of the type designed by them across the Cuyaguateje River. It would, however, be too simple to assume that the esthetic qualities of a bridge can be measured by the depth-to-span ratio only. Even if a hyppar bridge is somewhat deeper at midspan, I feel that it could still compete quite successfully with any other type of bridge on esthetic grounds. If it is then assumed that a hyppar bridge is properly designed with sufficient depth at all sections, the disadvantages due to too small a depth are eliminated.

As for the lever arm of the prestressing steel at midspan, the center of gravity can be easily placed at 8 or 9 in. from the bottom of the section. In

box girders with a square cross section, such a low placing can be achieved only by arranging the cables next to each other inside the box, as in the Cuyaguateje River Bridge. This favorable spacing is then achieved at the cost of not bonding the steel to the concrete. The favorable placing of the reinforcing at midspan in a hyppar bridge, combined with a high placing of the centroid of the section, results in a great economy in steel at midspan.

If it is then realized that only 50 percent of this steel has to be carried over the interior supports and that this steel can be placed close under the deck slab, it will be apparent that the center of gravity of the steel at an interior support is not nearly as low as would be expected at first observation.

In order to increase the lever arm of the steel at sections with negative moments, it might be advantageous to increase the thickness of the bottom of such sections. This can be easily accomplished as is apparent from Fig. 7. No additional formwork would be required. The section modulus of the bottom of the section would then, of course, also be increased.

As for the variable deck overhang, I would suggest to reinforce all precast slabs in an identical manner. Additional steel can be placed conveniently in that part of the concrete which is to be cast in place.

The amount of the overhang of the slab at midspan would be less than Messrs. Saenz and Martin estimate. If I follow their assumption that the depth of a bridge similar to the Cuyaguateje River Bridge would be 12 ft at the supports, and if I choose for the depth at midspan 6 ft 9 in. and for the barrel shape the hyperbolic paraboloid, then the maximum overhang at midspan would be 5 ft instead of 8 ft.

In my opinion the placement of the steel in the curved part of the section is simple, since all transverse cables can be directly placed on the forms. The longitudinal cables are straight so that their correct position can be easily shown by fixing just the two ends rather than indicating the changing elevation of each cable in different sections.

I agree with Mr. Müllersdorf that the sections near the interior supports will, in most cases, prove to be the governing factor in the design of a hyppar bridge. As pointed out above, however, these sections can quite easily be strengthened on the underside.

Friction in the cables will be of minor magnitude for all practical cases. For spans up to 400 ft, for instance, the maximum cable length would be approximately 300 ft.

In connection with the remark that "in statically indeterminate systems the efficiency with straight cables is usually less than with curved cables," it should be noted that it is one of the special features of a hyppar bridge that the effect of curved cables can be achieved with the use of straight cables.

The danger of settlement of falsework could, in some cases, be an argument against the use of a hyppar bridge or any other cast-in-place structure. The method as advocated by Mr. Müllersdorf provides beyond any doubt a good solution to this particular problem. It might also be worthwhile to investigate for such cases the possibility of using grout concrete. By so doing, the coarse

aggregate, which constitutes the greater part of the weight of the structure, would be placed first. Corrections in the alignment of the forms could then be carried out, after which the grout would be pumped.

If desired, haunches over momentless supports can easily be avoided without altering the spans.

Regarding fabrication of the deck slabs, it should be noted that the width of the deck is constant. The variable width of the box section does not affect the forms for the precast deck slabs. All that is necessary is to place the anchorages for transverse cables at varying points in these forms.

When designing a hyppar bridge, the designer will, as with all new developments, encounter difficulties. I am, however, confident that a most satisfactory and highly pleasing solution can be found.

The most decisive factor in the realization of a hyppar bridge will be whether the formwork can be made economically or not. When using the hyperbolic paraboloid, all curved supports follow the same parabolic shape. Some sort of series production can therefore be employed. Once these curved supports are in place, the forms are practically finished. The straight members that run over the supports may be of any length over a certain minimum. The plywood sheets are placed imbricately over the straight supports. No craftsmanship is required here.

I do thank again the gentlemen who, by their critical remarks, have helped further to clarify the merits of the hyppar bridge.

Discussion of a paper by A. F. Al-Alusi:

Diagonal Tension Strength of Reinforced Concrete T-Beams with Varying Shear Span*

By JO DEAN MORROW and CHARLES S. WHITNEY

By JO DEAN MORROW†

Data reported by the author on diagonal tension cracking of reinforced concrete T-beams were analyzed using equations developed by the writer and I. M. Viest in a recent ACI paper.‡ Data for the author's T-beams are shown as triangles in Fig. A. Also contained in the figure, as circles, are 71 stub beams and knee frames from the investigation just cited. All had rectangular cross-sections. The T-beams were treated as if they were rectangular sections the width of the stem of the tee.

Agreement between the diagonal tension cracking loads of the two types of beams is surprisingly good. Of the 25 T-beams reported by the author, the

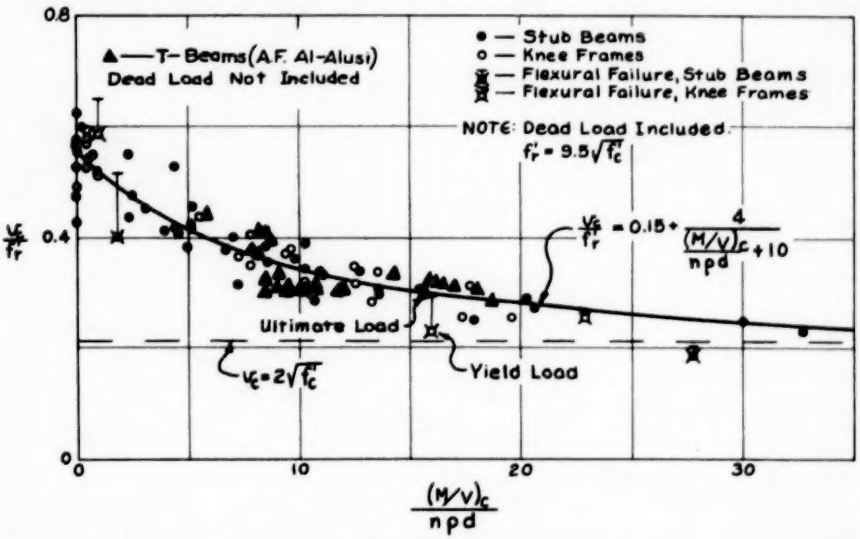

Fig. A—Comparison of diagonal tension cracking load for T-beams and rectangular beams

*ACI JOURNAL, May 1957, *Proc.* V. 53, p. 1067. Disc. 53-61 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 29, No. 6, Dec. 1957, Part 2, *Proceedings* V. 53.

†Assistant Professor of Theoretical and Applied Mechanics, University of Illinois, Urbana, Ill.

‡Morrow, JoDean, and Viest, I. M., "Shear Strength of Reinforced Concrete Frame Members Without Web Reinforcement," ACI JOURNAL, Mar. 1957, *Proc.* V. 53, pp. 833-869.

cracking load of 17 was predicted within ± 10 percent. The greatest deviation was only 20 percent low, and the over-all average of the ratio of the test over calculated cracking loads was 0.97. Had the dead load of the T-beams been included as it was for the stub beams and knee frames, the agreement would be even better.

It may be concluded that the diagonal tension cracking load for T-beams and rectangular beams may be predicted from the equation shown in Fig. A provided the T-beam is considered equivalent to a rectangular beam with the width equal to the stem of the tee.

By CHARLES S. WHITNEY*

The writer has presented† an equation for the ultimate strength of beams under concentrated load (without shear reinforcement) as follows:

$$v_u = 50 + 0.26 \frac{M_u}{d^2} \sqrt{\frac{d}{l_s}}$$

in which v_u is the unit shear at the diagonal cracking load, M_u is the ultimate moment capacity of the section per in. width, and l_s is the shear span.

According to this, the shear strength is principally dependent on the flexural reinforcement and only to a minor degree on the concrete strength f_c', except when the beam is over-reinforced.

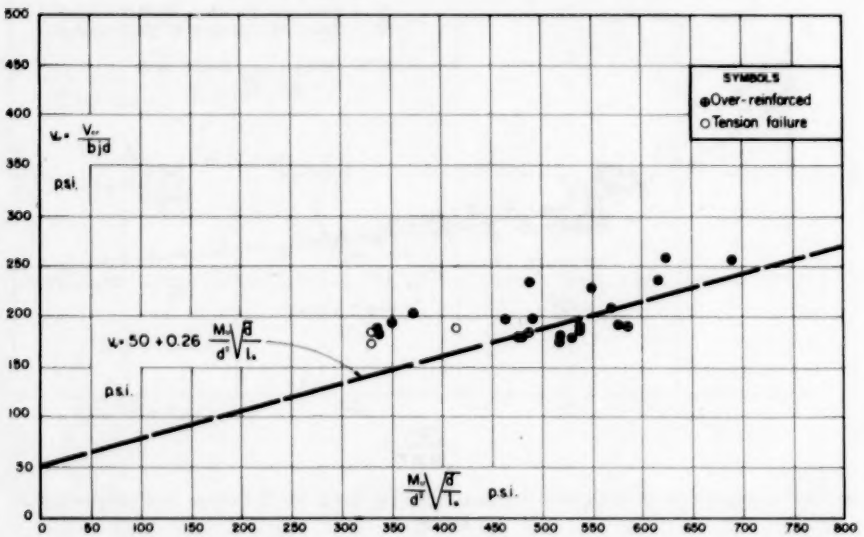

Fig. B—Author's test results (plotted points) compared with Whitney equation (dashed line)

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†Whitney, Charles S., "Ultimate Strength of Reinforced Concrete Flat Slabs, Footings, Beams, and Frame Members Without Shear Reinforcement," ACI JOURNAL, Oct. 1957, *Proc.* V. 54, pp. 265-298.

TABLE A

Beam No.	$\frac{M_u}{d^2}\frac{d}{l_s}$ psi	v_{cr}, psi	Beam No.	$\frac{M_u}{d^2}\frac{d}{l_s}$ psi	v_{cr}, psi	Beam No.	$\frac{M_u}{d^2}\frac{d}{l_s}$ psi	v_{cr}, psi
6	489	228	13	575	189	16	540	192
12	692	255	5	339	185	17	488	197
11	615	237	18	527	177	8	477	187
2	373	203	1	329	185	19	588	186
21	622	256	22	330	174	25	480	178
15	550	226	7	520	178	9	460	198
3	348	189	24	540	186	20	516	176
10	570	205				14	412	189
4	349	194				23	488	183

Table A and Fig. B show that Mr. Al-Alusi's data agree quite well with this equation. Beams 1, 22, and 14, failed in moment tension. The low strength of Beam 19 may well be due to the fact that it was over-reinforced and the crowding of the bars into a 3-in. stem may have induced bond failure. This theory is supported by the fact that the beams with less steel showed comparatively higher shear strength.

This analysis may indicate that the author's tests are too limited in scope to warrant his primary conclusions.

Discussion of a paper by Roy R. Clark:

Mass Concrete Control in Detroit Dam*

By CLARENCE RAWHOUSER, J. A. RHODES, DONALD P. THAYER, CHARLES L. TOWNSEND, and AUTHOR

By CLARENCE RAWHOUSER†

The author has made an excellent presentation of the results of specification requirements and temperature control used to prevent cracking of the concrete of Detroit Dam. The success of the procedure is another large step in advancement of a technique which at the time of its trial was far beyond the realm of experience. The cross section of Detroit Dam is much larger than any previously built without longitudinal contraction joints.

Elements which favored the adoption of low placing temperature as the principal measure of control were the relatively low ambient temperatures during the summer time, very short winters with temperatures not too far below freezing, and the design decision not to grout the contraction joints. There are not many conduits and penstocks through the section of the dam where maximum temperatures over 80 F were attained, which would act as large cooling pipes, but some of those may account for the relatively rapid drop in average temperatures of the massive sections of the dam which will be referred to later.

Experience at Detroit Dam has demonstrated that low placing temperature is the most effective measure available for placement of very long lifts on rock restraint without crack development while the top of the lift is exposed during construction. Complete absence of cracking on the tops and sides of large blocks even during winter exposures is an accomplishment that can be claimed for very few concrete dams. That it has been done effectively at Detroit Dam on blocks as long as 334 ft far surpasses any previous experience and definitely points the way to future application.

The author reports very favorable temperature variations from one zone to another at all times throughout the greater part of the structure. Under these conditions analysis of temperature strains on the basis of variation from maximum recorded to an assumed final stable temperature without verification from any observational strain data and with computations of factors of safety against cracking to one and two decimal places is a mathematical exercise that has very little meaning.

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The temperature declines of *N2*, *N3*, and *N4* (Fig. 5) seem either to have been weighted by thermometers relatively close to the surface or to have been affected by the conduits and penstocks through the dam for the concrete to have cooled during the 4 years 1952-1955 at so rapid and uniform a rate. The center of Zone 2 is about 120 ft from the upstream or downstream face. It might be expected, therefore, that the time required to cool from 67 F to 57 F under the given assumptions would be from two to three times as long as the records indicate.

The cost data presented are most welcome to engineers engaged in similar work. So far, very few data on cost of cooling by either embedded pipe or precooling method have been published. Efforts made by the writer to obtain such data from other jobs where precooling has been done have indicated that the costs generally are so intermingled with other costs of concrete production that cost of cooling could not be ascertained.

In this connection, Table A presented by the writer at a conference for construction engineers of the U. S. Bureau of Reclamation at Denver, Colo., in 1950 may be of interest. It shows the cooling costs for various dams cooled by embedded pipe systems constructed or under construction by the Bureau of Reclamation at that time. For dams that were not completed at that time, the costs were estimated from the contractors' bids. The year of the contract for each dam is listed to give the relation of cost with time; the volume of concrete cooled in each case is given, as it is reasonable to expect that unit cost for a large job might be less than for a small one. The cost in every case includes all labor and material chargeable to cooling, the purchase of tubing and fittings, installation costs, contract labor charges for cooling, for grouting the cooling tubing, etc.

It is not clear in the author's data for Detroit Dam whether the extra cost of $0.35 per bbl for Type IV cement over Type II that might otherwise have

TABLE A—COOLING COSTS FOR VARIOUS DAMS

Dam	Year	Volume cooled, cu yd	Cooling cost per cu yd
Hoover	1931	3,288,000	$0.25
Parker	1934	291,000	0.20
Seminoe	1935	176,000	0.33
Grand Coulee (1)	1935	4,237,000	0.13
(2)	1937	5,605,000	0.18
Shasta	1938	6,015,000	0.21
Friant	1939	2,022,000	0.30
Marshall Ford (Enlargement)	1940	656,000	0.55
Keswick	1941	71,600	0.64
Davis	1945	240,000	0.90
Kortes	1946	82,000	0.43*
Angostura	1946	170,000	1.00*
Hungry Horse	1948	2,930,000	0.68*
Canyon Ferry	1949	360,000	0.77*

*Estimated from contractors' bids.

been used has been included in the cost of cooling. If not, it would be of interest to know how much this item would add to the cost per cu yd for temperature control of Detroit Dam.

By J. A. RHODES*

The importance of volume changes resulting from heat generated by the hydration of cement in massive concrete structures has been recognized for many years, and was probably first actively and thoroughly considered in the United States during the design and construction of the Boulder Canyon project by the U. S. Bureau of Reclamation about 1930. Basic principles explored and reported by the USBR at that time, supplemented and extended by the TVA and other engineers during the subsequent decade, include the influence of ambient air temperatures on heat loss, the temperature history of the concrete, and temperature distribution and variation within the structure as it approaches its stable cyclic thermal condition. These characteristics of mass concrete structures were well established and documented prior to 1940, and confirmed by the Corps of Engineers by measurements of temperatures in such structures since that date.

In spite of the extensive studies carried out by the Corps of Engineers in the design, and the experience gained from observations made during construction, of mass concrete structures, it is a matter of record that thermal cracks often develop where and when they apparently should not. On the other hand, many mass concrete structures constructed by the Corps of Engineers prior to Detroit Dam suffered little or no thermal cracking. From this it must be concluded that concrete temperature control design is not yet refined to the degree that other structural design methods have attained, and that post-construction theoretical analyses intended to prove that a structure cracked (or did not crack) as expected (or hoped) are suspect and frequently misleading to engineers not entirely familiar with all aspects of the temperature control problem.

A presentation of the design study results which led to the decision to construct Detroit Dam with no vertical joints parallel to the dam axis, the detailed mathematical justification for the rejection of 89 F as an unacceptable maximum concrete temperature, and the accurate stress analysis referred to by the author in support of his strain calculations would be interesting, informative, and extremely helpful to designers of future mass concrete structures.

Triaxial strains

The simplified approach to potential thermal strains, that of an element tending to experience length change in one direction only, ignores the triaxial conditions which must exist within the lower portion of a large concrete mass.

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Temperature variations cause volumetric changes in a mass, i.e., in the direction of all three major axes, and in the analysis of a cube or element the effect of adjoining elements must be considered in arriving at a realistic determination of stress or strain conditions.

Restraint is also subject to the complications of three-dimensional strains, a 100 percent value for uniaxial restraint being equivalent theoretically[1] to about 150 percent triaxial restraint for concrete with a Poisson's ratio of the usual order. With a higher value of Poisson's ratio, which accurate experimentation has shown may exist in early age concrete, the triaxial restraint would exceed the 150 percent value.

Beam flexural tests

Determination of the extensibility of mass concrete in a structure from measurement of ultimate strain in a test beam is open to question. First, characteristics of the prototype concrete (cement content, maximum aggregate size, curing history, and moisture content changes, shrinkage, and growth) are difficult if not impossible to reproduce in a laboratory test. Secondly, it was found[2] prior to 1935 that "substantially higher maximum fiber strains could be developed in flexural compression than in cylinder tests," and in the same ACI JOURNAL Hognestad stated[3] that "concrete in concentric compression will show signs of crushing shortly after the maximum stress is reached at strains of 1.5 to 2 thousandths, while crushing takes place at the compression face of members subject to flexure at strains of 3 to 5 thousandths." Talbot noted this same condition as early as 1904. Whether this is due to stress adjustments and/or shifting of the neutral axis as rupture loads are approached or to other circumstances is not discussed.

Unless, therefore, it can be demonstrated that a similar relationship does not prevail between ultimate tensile strains in flexure and direct tension, the use of a numerical value for ultimate tensile strain taken directly from a flexural test is not a valid measure of the resistance of concrete to thermal tensile strains.

Analysis of temperature strains

As the author has stated, several assumptions were made in his arithmetical approach to factors of safety against thermal cracking, and most of them were of such a nature that end results of the analysis appear to be conservative. It should be pointed out that other assumptions were made or conditions arbitrarily selected which affected the calculated results in an opposite manner. The effect of triaxial strains and triaxial restraint on stress developed in the interior of the structure was not considered and the applicability of ultimate tensile strain limits derived from flexural tests to other types of loading was assumed by the author to be permissible. Pursuing the concrete extensibility characteristics still further, Jensen presented data[4] showing that the ultimate strain of low strength concrete beams (2000-psi concrete) is 170 percent and 145 percent greater than similar beams of 4000-psi and 3000-psi

concrete respectively. In applying the concept of extensibility to concrete, an ultimate strain limit for concrete during a specific loading cycle must be modified for use at later ages as a result of the continued increase in strength and/or elastic modulus of the concrete with age. Consideration of this feature alone would indicate that the factor of safety for the ultimate strain in Zone 1, calculated by the author's method, is about 1.25 rather than approximately 2.0 or greater.

It is clear that several unfavorable assumptions are involved in the strain analysis, in addition to the conservative assumptions specifically pointed out by the author as tending to increase his calculated factor of safety, and the net result of all cannot be established until the relative effects of each are evaluated.

Temperature gradients

Examination of the concrete temperature records for thermometers in Zone 1 of Detroit Dam failed to confirm the rates of temperature drop reported by the author. If by the term "first decline" the author was referring to the temperature drop resulting from heat loss during exposure of a lift of concrete prior to placement of a covering lift, the average rate of temperature decline appears to be 1.9 F per day for the 20 thermometer histories showing distinct breaks in slope prior to subsequent rises to higher temperatures. The maximum rate of decline observed at an individual thermometer location was 4.3 F per day. The remaining 18 thermometers in Zone 1 were either close to an exposed face, near the top of a lift exposed for a considerable period of time, or were clearly affected by the temporary flooding of the cofferdam following placement of the final lift in Zone 1 of Block 20, and temperature histories were not typical of conditions within the concrete mass.

The author's values for Zone 1 can be more nearly checked if the "first decline" is interpreted to mean the rate of temperature drop during the first 30 days following the time of maximum indicated temperature at the thermometer location. With this interpretation, however, rates of temperature decline in Zone 4 as indicated by the project temperature records do not agree with the author's figures. Clarification as to the procedure followed to obtain the reported results would be desirable.

Rates of measured temperature decline during the first 30 days after maximum temperature in mass concrete, based on typical thermometers located at points far removed from exposed surfaces, are shown in the following tabulation covering several Corps of Engineers projects.

Dam	Rate of decline, deg F per day
Chief Joseph	0.10
Clark Hill	0.08
Harlan County	0.10
John H. Kerr	0.07
Pine Flat	0.03
Table Rock	0.05

From this tabulation it appears that the rate of temperature drop experienced at Detroit Dam, rather than being exceptionally low, was unusually high.

SUMMARY

Mass concrete temperature control practices followed at Detroit Dam were not unique, in that each of the control features (except the selective use of Types II and IV cement) had been adopted previously in the construction of other mass concrete structures.

Although temperature histories in a mass concrete structure are estimated comparatively easily during the design stage with reasonable confidence, stress or strain conditions cannot be evaluated accurately by a simple arithmetic analysis from either estimated or observed concrete temperatures.

Results at Detroit Dam demonstrate that a reasonably crack-free mass concrete structure can be constructed under the climatological conditions prevailing at that location, using the aggregates available and the temperature control practices specified, and following the particular construction schedule adopted by the contractor. This is the most dependable conclusion supported by the Detroit Dam study.

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By DONALD P. THAYER*

The author is to be highly commended for an able presentation of the methods of mass concrete temperature control employed in construction of Detroit Dam and for his analysis of the results thereby achieved. Construction of monoliths of the size involved in this dam substantially free of cracking is a noteworthy achievement, and the methods used are of particular interest to engineers contemplating similar structures.

It is quite reassuring to know that a structure designed as a monolith is, upon construction, truly monolithic and not cut at random and unknown places by thermal cracks. Premeditated cutting of the concrete at intervals by contraction joints which are later to be closed by pressure grouting is at

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best a costly and sometimes questionable operation. Any method which permits the placing of a monolith without joints, intentional or otherwise, is certainly worthy of the most careful consideration. The objection is often raised that elaborate requirements for temperature control are inordinately expensive; however, the added cost quoted by Mr. Clark is quite nominal and should allay any apprehension on this score.

The use of steel forms on the Detroit Dam is of special interest to the writer. Originally conceived in connection with this job as a means for eliminating thermal shock upon removal of insulating wood forms, it opens a new avenue for concrete temperature control. During the summer months the average ambient temperature may be as much as 25 F above the initial concrete placing temperature, resulting in a heat gain to the concrete during the first 3 or 4 days. A logical extension of the concept of steel forms is to utilize artificial cooling of the forms to maintain the surface temperature at or near the initial concrete temperature. This would operate both to eliminate the heat gain from the atmosphere through the side forms and to insure that the no-stress state of the concrete would be attained at a lower temperature, thereby minimizing the tensile stresses resulting from subsequent exposure to the annual temperature variations.

This method is being considered in the design of the Oroville Dam on the Feather River in California, and studies along this line are being made by Roy W. Carlson, Berkeley consulting engineer, together with necessary determination of elastic-plastic properties of the concrete at the University of California. No conclusions are available at this writing; however, preliminary investigations are promising and it is hoped that monoliths nearly twice the linear dimension of those at Detroit Dam may be constructed. At the Oroville site there is the advantage of more favorable ambient temperature conditions.

An obvious fact should be remembered; direct comparison between two dams is never possible due to the many peculiarities existing at each site. Differences in cement content required, differences in thermal properties of materials, differences in daily and annual temperature cycles at the site, and many other peculiarities all preclude direct comparison. Nevertheless, by separately considering the effect of these factors, pertinent comparisons may be made and for this reason a paper such as Mr. Clark's is of especial value and interest to engineers engaged in the design of other projects.

Another factor of significance which is often disregarded should be mentioned. The fact is that precooling concrete, in addition to the primary purpose of controlling the subsequent thermal history, actually produces *better* concrete. Reference is made to Fig. 10, in the *Concrete Manual** which indicates that concrete mixed at 40 F has about 10 percent greater strength than concrete mixed at 85 F.

**Concrete Manual*, U. S. Bureau of Reclamation, 1956 Edition, p. 21.

By CHARLES L. TOWNSEND†

Mr. Clark has covered, with an excellent approach, an important feature in the design of large concrete dams. Many theoretical studies, equations, graphs, nomographs, and the like have been prepared to aid the engineer in planning the temperature control of a massive concrete structure, but too many variables are involved to accept theoretical heat flow results at face value. Such variables include the heat generating characteristics of a single type of cement which may vary considerably, the thermal conductivity of concrete which may vary as an aggregate deposit is worked from one end to the other, air and water temperature which occur during the construction period, and the effects of forms, curing, solar radiation, and insulation, if used. Mr. Clark has taken into consideration these variables and has arrived at conclusions which are based on theory but tempered with experience and good judgment.

In describing the cooling of concrete through the embedded pipe system, Mr. Clark stated that the concrete is *sometimes* cooled below the ultimate expected temperature to allow for persistent heating as the concrete continues to hydrate at later ages. Actually, it is *normal* practice to cool below the ultimate expected temperature prior to grouting the contraction joints, and the primary purpose of this sub-cooling is to obtain the benefit of a temperature rise. Such a temperature rise will not only assure that the contraction joints remain closed, but will add a "prestressed" condition which will reduce certain tensile stresses which would ordinarily exist. The writer believes that the subsequent temperature rise is caused primarily by the concrete assuming a final stable temperature dependent on exposure conditions rather than from the relatively small amount of heat developed in the concrete by the continued hydration of the cement.

Many designers feel that conflicting methods of temperature control exist because the Corps of Engineers primarily uses a precooling form of temperature control while the Bureau of Reclamation primarily uses an embedded pipe system. The writer agrees with Mr. Clark and his statement that both methods are aimed at producing a monolithic structure. Each method has its advantages and disadvantages, and, under certain circumstances, a combination of both methods may be required to obtain a desired result. Relatively small gravity-type structures require little temperature control, while the larger gravity-type structure which measures several hundred feet in thickness requires a high degree of control if cracks parallel to the axis of the dam are to be prevented. These uncontrolled cracks are related to both the amount and rate of temperature drop from the maximum concrete temperature to the final temperature, and the precooling method is especially fitted for this because it lowers the maximum concrete temperature (and thereby the temperature drop to the final temperature) and it allows the concrete to cool slowly over a period of years. A gradual opening of the contraction joints

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is of little concern from the stability standpoint because each block is designed to stand by itself. On the other hand, dams which are designed to carry a large share of the load to the abutments by arch action must have the contraction joints closed for such arch action to take place. If the water loads were to be applied to an arch dam with open contraction joints, the individual blocks would act as cantilevers and deflect downstream until closure of the joints was effected. Depending on the curvature, height, and thickness of section, this deflection could cause serious structural cracking near the base of the cantilevers at the upstream face of the dam before the joints closed.

A second consideration may be the time when the loads are to be applied. The majority of the dams constructed by the Bureau of Reclamation require an early storage of water for irrigation. This requirement means that the contraction joints of an arch dam must be permanently closed during or immediately after construction. For arch dams, therefore, the use of an embedded pipe cooling system is necessary, and possibly in conjunction with precooling. This type of temperature control not only reduces the maximum temperature but also provides the means whereby the concrete can be cooled to any desired temperature for contraction joint grouting. The cooling for contraction joint grouting is normally accomplished not earlier than 6 months after placing and after the concrete has attained most of its strength. Nevertheless, the cooling operation should be carefully controlled to prevent its causing cracks by lowering the temperature too rapidly.

The results obtained at Detroit Dam show that the methods of concrete control were very satisfactory. The writer's only comments are in regard to the use of Type IV cement. Mr. Clark estimated the mean annual temperature at the site to be 50 F, and directed all treatment toward this as the final temperature of the dam. Fig. 5, however, would indicate that a final concrete temperature of 55 F is being obtained. From this it appears that a Type II cement could have been used with a 50-F placing temperature to obtain a maximum temperature of 84-85 F. The subsequent drop to 55 F would then have been within the 30-F drop which Mr. Clark set up in his design criteria. It would also appear that the 30-F drop used as a design criterion could have been increased to perhaps a 35-F drop in the upper part of the dam where the foundation restraint factor is lower. With this in mind, Type IV cement would only have been required near the rock foundations, and a Type II cement could have been used throughout the remainder of the dam.

AUTHOR'S CLOSURE

The author appreciates the interest aroused and the thoughtful discussions of the four readers. The principal object in presenting the text was to encourage others to pursue the question of mass concrete control—a neglected subject so far as publication of ideas and facts are concerned. The author was responsible for concrete temperature calculations of the Bonneville Dam

in 1934 with the able assistance of Roy W. Carlson. Strain meters and resistance thermometers were installed in the Bonneville Dam and some of the readings were published in 1937.* Not enough has been published on this subject since that time.

The comments that rapid cooling took the place in the Detroit Dam concrete, especially in Zone 1, may be explained by the fact that in this zone the foundation rock was very rough with numerous deep recesses which increased the rock surface area exposed to the concrete more than for a flat surface. The rock thus absorbed the heat and caused more rapid cooling, although the 12 F drop in 39 days is certainly more favorable than the rapid drop in temperature when mass concrete temperature is lowered 50 deg or more in the same time by forced cooling. From Fig. 5, Curves *N1* and *N2* indicate that the rock temperature followed the concrete temperature in Zone 1 so closely that restraint was reduced and concrete strains were undoubtedly less than that indicated in the text, therefore, a greater factor of safety against cracking.

Mr. Rhodes offers the opinion that flexural tests of unreinforced concrete beams do not give strain results that represent tensile conditions at the base of the dam and that a triaxial effect must be considered. There may be some basis for this concept, theoretically; yet the triaxial tests so far developed relate to compression only. The author knows of no correlation between compressive strength resulting from triaxial tests and the tensile strength of the same concrete. This was given consideration in preparing the text and it is hoped that it may arouse interest in the problem of determining the safe strength of mass concrete in tension at the base of a large dam. The element under consideration was 50 ft wide and 334 ft long. From a practical standpoint when considering tensile strains resulting from a reduction in temperature, it is not hard to believe that the strain normal to the long axis of the dam alone should be considered until more accurate data on tensile strengths are available. If Mr. Rhodes has any special knowledge on this subject, or knows of dependable data developed from measured tensile strains, or stress, it is his professional duty to publish the same. Until then, tensile strain measured as described is the best approach known to the author. The concrete temperatures for each zone were determined by averaging all thermometer results except those near the surface. The surface concrete was discussed separately.

Mr. Rhodes includes one remark in his discussion which indicates an inadequate understanding of the design and specification requirements of the Detroit Dam. He states, "following the particular construction schedule adopted by the contractor." The contractor's construction schedule had nothing whatever to do with the favorable results obtained. The specifications included all of the special provisions described for control features. The contractor had no choice but to follow them, which he did in a satisfactory

*Clark, R. R., and Brown, H. E., Jr., "Portland-Pozzolan Cement as Used in the Bonneville Spillway Dam," ACI JOURNAL, Jan.-Feb. 1937, *Proc.* V. 33, pp. 183-221.

manner. No alternates to the control measures were proposed or approved. This remark is therefore not pertinent.

The cost data on cooling by embedded pipes offered by Mr. Rawhouser is helpful. Rapid cooling until the temperature drops five times the decrease at Detroit Dam is not considered equal to the slower natural cooling experienced especially in Zone 1 at the base of the dam. The cooling costs presented in the text were obtained by collaboration with the contractor who made a detailed cost analysis available to the author with the statement that this was his first attempt and that costs could be greatly reduced a second time.

In conclusion, it is hoped that this paper will arouse others to publish their data and observations on the all-important subject of mass concrete control. When applied to a large dam, the subject is essentially a structural one and the solution can only be found by that approach. Too often, those concerned with the subject are "concrete engineers," and have an inadequate conception of the basic factors that must be observed to successfully control mass concrete in a large structure.

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ACI *Proceedings* V. 53 is complete with Part 2 December JOURNAL—How to assemble

When JOURNALS for July 1956 through June 1957 are pulled apart (after removing wire stitches), and covers, masthead (page just inside front cover), and *News Letter* sections are all discarded, remaining parts may be assembled with the parts of December Part 2 to complete *Proceedings* V. 53.

Start with title page and contents taken from Part 2 and follow with the issues separated as noted above from July to June in chronological order. Then add discussion pages and indexes taken from this Part 2. All discussion comes at the end of this volume instead of following each paper to which it pertains.

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